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THE LONDON, EDINBURGH, AND DUBLIN PHILOSOPHICAL MAGAZINE AND JOURNAL OF SCIENCE.

CONDUCTED BY

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AND

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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

VOL. XLVI.—FOURTH SERIES.

JULY—DECEMBER 1873.

LONDON.

TAYLOR AND FRANCIS, RED LION COURT, FLEET STREET,

Printers and Publishers to the University of London;

SOLD BY LONGMANS, GREEN, READER, AND DYER; KENT AND CO.; SIMPKIN, MARSHALL, AND CO.; AND WHITTAKER AND CO.;—AND BY ADAM AND CHARLES BLACK, AND THOMAS CLARK, EDINBURGH; SMITH AND SON, GLASGOW:— HODGES, FOSTER, AND CO., DUBLIN:—PUTNAM, NEW YORK:—AND ASHER AND CO., BERLIN.

"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JULY 1873.

I. *On the Relations between the characteristic Quantities occurring in Central Motions.* *By* R. Clausius*.

IN a memoir published last year†, I derived for the motion of a material point about a fixed centre of attraction, and for the motion of two material points around each other, some new relations between period of revolution, *vis viva*, ergal, and energy, which have the indispensable condition that the motions take place in closed paths. As, however, this condition is generally fulfilled only with particular kinds of attractive forces, it appeared to me important to extend the same method of treating the subject to the case in which the paths do not form closed curves; and I take leave to communicate the following results of this investigation.

1. Two new equations advanced by me for stationary motions serve as the basis of the inquiry. I will therefore first quote these equations; and as their connexion with equations previously known has recently been mooted in various quarters, it will perhaps be advisable also briefly to discuss this point, in order on the one hand to make evident the connexion, and on the other hand the difference.

Given a freely movable point with the mass m, which at the time t has the coordinates x, y, z, and is acted on by a force the components of which, taken in the directions of the coordinates,

* Translated from a separate copy, communicated by the Author, from the *Nachrichten der Königl. Gesellsch. der Wiss. zu Göttingen* of Dec. 25, 1872.

† *Nachr. d. K. Ges. d. Wiss.* May 24, 1871; *Math. Ann. von* Clebsch *und* Neumann, vol. iv. p. 231; Phil. Mag. November 1871.

are X, Y, Z; then from the differential equation of motion,

$$m\frac{d^2x}{dt^2}=\mathrm{X},$$

can very readily be derived the following equation:—

$$\frac{m}{2}\left(\frac{dx}{dt}\right)^2=-\frac{1}{2}\mathrm{X}x+\frac{m}{4}\frac{d^2(x^2)}{dt^2}. \quad . \quad . \quad . \quad . \quad (1)$$

This equation I have applied to the case in which the motion is stationary—that is, where the coordinates and velocities do not continually change in the same direction, but only vary within certain limits*. In this case the mean value, referred to one period of the motion or to a very long time, of the differential coefficient of the second order $\frac{d^2(x^2)}{dt^2}=0$. Hence, if we indicate the mean values of the two other quantities occurring in the equation by putting a horizontal stroke above their symbols, the equation becomes

$$\frac{m}{2}\overline{\left(\frac{dx}{dt}\right)^2}=-\frac{1}{2}\overline{\mathrm{X}x}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Equations (1) and (2) hold, of course, in the same form, also for both the other coordinates. If, further, instead of a single freely movable point, we have a whole system of such points, the same equations hold for each point of this system. We can therefore forthwith extend them so that they shall refer to all three coordinates and the whole system of points. If, namely, we employ the letter r for the radius vector of a point, and the letter v for its velocity, which letters can have different indices for the different points, the two equations will be transformed into:—

$$\Sigma\frac{m}{2}v^2=-\frac{1}{2}\Sigma(\mathrm{X}x+\mathrm{Y}y+\mathrm{Z}z)+\frac{1}{4}\frac{d^2(\Sigma mr^2)}{dt^2}, \quad . \quad . \quad (1a)$$

$$\Sigma\frac{m}{2}\overline{v^2}=-\frac{1}{2}\Sigma\overline{(\mathrm{X}x+\mathrm{Y}y+\mathrm{Z}z)}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

Equation (2), in its amplified form (2a), is the *first* of my above-mentioned two equations. The quantity found on the right-hand side of (2a) I have named the *virial* of the system, and expressed the sense of the equation in the following proposition:—*The mean* vis viva *of the system is equal to its virial.* But at the same time I remarked that the proposition is true not merely for the whole system of points, and for the three coordi-

* *Sitzungsberichte der Niederrhein. Gesellschaft für Natur- und Heilkunde* 1870, p. 114; Pogg. *Ann.* vol. cxli. p. 124; Phil. Mag. S. 4. vol. xl. p. 126.

nates together, but also for each point and each coordinate singly. Naming the quantity $\frac{m}{2}\left(\frac{dx}{dt}\right)^2$ the *vis viva* of the point relative to the x-direction, and in like manner the quantity $-\frac{1}{2}\overline{Xx}$ its *virial* with respect to the x-direction, and considering that the x- may be any direction we choose, we can express the proposition in the following terms:—*For every point the mean* vis viva *relative to any direction is equal to the* virial *relative to the same direction.*

Now I have been reminded that an equation advanced by Jacobi and another by Lipschitz stand in connexion with the considerations I have instituted.

Jacobi's equation is found in Crelle's *Journal*, vol. xvii. p. 121, and in Jacobi's *Vorlesungen über Dynamik*, p. 22, where it is given as equation (2). It is there assumed that the forces acting in the system have a force-function, and, still more specially, that this force-function U is a homogeneous function of the kth dimension; and the equation relating to it is:—

$$\frac{d^2(\Sigma m_i r_i^2)}{dt^2} = (2k+4)\mathrm{U} + 4h,$$

in which h is the additive constant belonging to the force-function. It is evident that this equation can only be compared with equation (1 a), and not with (2a); and even from (1a) it is distinguished, on the one hand, by a very different form (as it does not include the *vis viva*), and on the other very essentially by holding only for a very limited class of forces, while (1a) is valid for all forces.

Lipschitz's equation is much more general (contained in his memoir "über einen algebraischen Typus der Bedingungen eines bewegten Massensystems"*). He likewise has under consideration a system of material points in motion, $P_1, P_2, \ldots P_n$, which, however, need not be free, but may be subject to conditions which are expressed by equations of a form there more closely defined. He assumes for each point a certain defined position, the coordinates of which are denoted for the point P_α by $a_\alpha, b_\alpha, c_\alpha$, while P_α itself at the time t has the coordinates $x_\alpha, y_\alpha, z_\alpha$. Further, besides the force-function U and the *vis viva* T of the entire system, he introduces a quantity G, which is determined by the following equation:—

$$\Sigma_\alpha m_\alpha\left[(x_\alpha-a_\alpha)^2+(y_\alpha-b_\alpha)^2+(z_\alpha-c_\alpha)^2\right]=2\mathrm{G};$$

* *Journ. für reine und angew. Math.* vol. lxvi.

and with the aid of this quantity he forms the following equation:—

$$\frac{d^2G}{dt^2}-2T=\Sigma_\alpha\left[(x_\alpha-a_\alpha)\frac{dU}{dx_\alpha}+(y_\alpha-b_\alpha)\frac{dU}{dy_\alpha}+(z_\alpha-c_\alpha)\frac{dU}{dz_\alpha}\right].$$

This is very similar to equation (1); in some relations it is more general. First, Lipschitz has given it a more general signification, as he advances it not merely for free systems, but also for systems which are subject to certain conditions; and, secondly, for each point a special defined position is assumed, which is subject to further arrangement, while in (1 *a*) the corresponding position marked out for each point is the initial point of the coordinates. In other relations, on the contrary, it is more limited, as in it the forces are presupposed to have a force-function, which is not the case in (1 *a*).

Further than this equation (numbered 6 in his memoir) the agreement of Lipschitz's considerations with mine does not extend, as from this point he gives quite another turn to the investigation. While even equation (1), which is more general than (1 *a*), I have applied to stationary motions, and for these have derived equation (2), Lipschitz proceeds as follows. First he gives to his equation another form, in that he eliminates from it the *vis viva* by means of the relation which subsists between this and the force-function. Then he goes on to further specialize his equation by making definite suppositions concerning the nature of the force-function, particularly this—that it is an algebraic homogeneous function of the elements $x_\alpha-a_\alpha, y_\alpha-b_\alpha, z_\alpha-c_\alpha$. For the cases limited by these presuppositions, he then inquires what condition is necessary and sufficient in order that the motion may be stable. This investigation is, on account of the subject on which it treats, as well as the treatment itself, of high interest; but it is altogether different from my considerations, and accordingly the result which I have derived, and expressed by the theorem of the virial, does not appear in it.

The circumstance that no one before me put forth this theorem, although equation (2) is so easily obtained from the fundamental equations of motion, must, I think, be accounted for by this, that hitherto they had less inducement to turn their attention to stationary motions as such. But since the newer view of the nature of heat has become current we have, in the theory of heat, to do with a stationary motion of the minutest constituents of bodies; and as we knew (and still know) very little about the nature of this motion, it lay near to draw at least the conclusion which could already be drawn from the condition that the motion is stationary; and indeed the theorem thus gained is of peculiar importance for the theory of heat.

2. As for my *second* equation, I will here quote it only in the form which it has for a single movable point, since that form is sufficient for the following considerations, and the extension of it to a system of any number of points would require analyses of too great a length.

This equation stands in connexion with those which express the proposition of least action and Hamilton's amplification of the same; yet in an essential point it is different from them, as will be immediately evident when I put here the two latter equations, so that all three may be seen together.

Let a material point move freely, under the influence of a force, from a given initial point to a given final point. Then, instead of this motion, imagine another motion of the material point taking place between the same two limits in a path infinitesimally changed. If, now, the force acting on the point has a force-function or (as I have proposed to name it) *ergal*, and if it is further assumed that in both motions the ergal is represented by the same function of the space-coordinates, and that the energy (the sum of the ergal and *vis viva*) has the same value in each, then the following equation (known as the expression of the proposition of least action) holds:—

$$\delta\int v^2 dt = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

If the time which the point requires for its motion be denoted by i, then this equation can also be written thus:—

$$\delta(\overline{v^2}i) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3a)$$

Now this equation is valid even when the limits between which the changed motion takes place are not the same as with the original motion, provided they fulfil the condition that the quantity

$$\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z$$

has the same value at the end of the motion as at the beginning. This condition is fulfilled, for example, when both motions are in closed paths and in each motion an entire revolution is considered, so that the point where the motion ends coincides with the point where it begins.

If, on the contrary, this condition relative to the limits were not fulfilled, for $(3a)$ the following equation would have to be substituted,

$$\delta(\overline{v^2}i) = \left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_1 - \left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_0, \quad (3b)$$

in which the indices $_0$ and $_1$ signify the initial and the final value of the bracketed terms. For the sake of simplicity we will, in

the following, always consider that condition fulfilled, and accordingly put the difference which stands on the right-hand side of the above equation $=0$, so that the equation will retain the form (3a).

The amplification introduced by Hamilton consists in the same value not being attributed to the energy in both motions, a change of energy being considered admissible, while yet the ergal with the changed motion must still be the same function of the space-coordinates as with the original motion. The equation given by Hamilton for this case* is the following, in which E signifies the energy:—

$$m\delta(\overline{v^2}i) = i\delta \mathrm{E}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

The earliest form of my equation† is

$$-\overline{(\mathrm{X}\delta x + \mathrm{Y}\delta y + \mathrm{Z}\delta z)} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta \log i. \quad . \quad . \quad (5)$$

In this equation the force acting on the point is not limited by any condition. Let us suppose, as before, that the force has an ergal, and designate it by U, fixing at the same time the positive and negative directions of the ergal so that the sum of the *vis viva* and the ergal during the motion is constant, then the equation becomes

$$\overline{\frac{d\mathrm{U}}{dx}\delta x + \frac{d\mathrm{U}}{dy}\delta y + \frac{d\mathrm{U}}{dz}\delta z} = \frac{m}{2}\delta\overline{v^2} + m\overline{v^2}\delta \log i; \quad . \quad (5a)$$

here, however, it is not assumed, as in Hamilton's equation, that the function (designated by U) of the space-coordinates is invariable, but this function may, on one motion passing into the other, undergo a change independent of the alteration of the coordinates. Let us imagine, for example, that the function contains any quantities whatever independent of the coordinates and therefore constant during the motion, these constants, in order that Hamilton's equation may hold, must have the same values when the motion is changed as they had with the original motion. On the contrary, this is not necessary for the validity of my equation, but with the transition from one motion to the other the constants may change their values. This gives to the sum

$$\frac{d\mathrm{U}}{dx}\delta x + \frac{d\mathrm{U}}{dy}\delta y + \frac{d\mathrm{U}}{dz}\delta z,$$

the mean value of which occurs in my equation (5a), a peculiar significance. We cannot replace it by the symbol $\delta\mathrm{U}$, if by this

* Thomson and Tait, 'Treatise of Natural Philosophy,' p. 235.

† *Sitzungsberichte der Niederrhein. Gesellsch. für Natur- und Heilkunde*, 1870, p. 174; Phil. Mag. S. 4. vol. xlii. p. 167.

we understand the complete variation of the ergal. That is to say, the complete variation must contain not merely the difference which is conditioned by the difference of the coordinates, but also that which arises from the change of the form of the function, consequently from the change (for example) of certain constants occurring in the function. If we designate these constants by c, c_1, &c., and their changed values by $c+\delta c$, $c_1+\delta c_1$, &c., and will employ the universal-variation symbol δU, my equation must be written thus:—

$$\delta\overline{U}-\frac{d\overline{U}}{dc}\delta c-\frac{d\overline{U}}{dc_1}\delta c_1-\&c.=\frac{m}{2}\delta\overline{v^2}+m\overline{v^2}\delta\log i. \quad (5\,b)$$

In order to gain a more convenient way of writing the equation, it will perhaps be advisable to introduce a special symbol for that part of the variation which relates only to the alteration of the coordinates, *e. g.* to employ a δ with an index, putting

$$\delta_1 U=\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z.$$

My equation then reads,

$$\delta_1\overline{U}=\frac{m}{2}\delta\overline{v^2}+m\overline{v^2}\delta\log i^*. \quad . \quad . \quad . \quad (5\,c)$$

3. Having made these preliminary remarks, we can now proceed to the discussion of central motions.

When we refer the motion of a point about a fixed centre of attraction to polar coordinates whose middle point coincides with that centre, two different processes present themselves for our consideration—the angular motion of the radius vector, and the motion of the point in the radius vector. The latter, so far as the whole motion generally is stationary, consists in alternate approach toward the centre and removal from it.

When the time expended in an approach and recession is equal to the period of revolution of the radius vector, after each revolution the moving point comes again to the same place, and from here commences a new revolution in the same path, and consequently we have a perpetual motion in a closed path. It is the same when during one revolution any whole number whatever of

* In my first memoir relative to this subject I have, it is true, replaced the sum on the left-hand side of my equation by the simple symbol $\delta\overline{U}$; but I have there expressly attached to the letter U a different signification from that here given to it—namely, by saying there, "let U denote the ergal *for the original motion.* The change in the form of the ergal which enters with the transition from one motion to the other I then expressed thus—by designating the ergal for the altered motion by $U+\mu V$, in which V denotes a second function of the coordinates, and μ an infinitely small constant.

approaches and recessions is accomplished. If, further, during one approach and recession several revolutions take place, or if, generally, the period of an approach and recession be merely commensurable with the period of a revolution, the point will, certainly not after each revolution, but yet after a certain number of revolutions, come again to the same place and then repeat in like manner the motion just completed, so that a closed path will result from several revolutions. If, on the contrary (which is the most general case), the period of approach and recession be incommensurable with the period of revolution of the radius vector, each successive revolution will take place in another path, and we shall then have no longer to do with a closed path.

Now, in order to apply my *first* equation to the central motion of a point, we can first give the virial a simpler form. If the force acting on the point is an attraction or repulsion proceeding from the origin of the coordinates, the intensity of which is represented by the function $F'(r)$, a positive value of the function denoting attraction, and a negative value repulsion, then is

$$-\frac{1}{2}(Xx+Yy+Zz)=\frac{1}{2}rF'(r),$$

and hence equation $(2a)$ changes into

$$\frac{m}{2}\overline{v^2}=\frac{1}{2}\overline{rF'(r)}. \qquad (6)$$

In this equation the distinction whether the path is closed or not is of no consequence. For its applicability it is merely necessary that the motion be *stationary*, so that the quantities v^2 and $rF'(r)$ shall not continually change in the same direction, but only oscillate within certain limits and hence have definite mean values.

It is not so with my *second* equation; in this occurs the period i of the motion under consideration. If, then, the motion takes place in a closed path and therefore regularly repeats itself in exactly the same manner, we can confine our attention to a single run through this path, and take the time required for it as the quantity i. I have treated of this case in my previous memoirs. But when the motion takes place in a path which is not closed, and therefore does not present any periods repeating themselves in the same manner, the contemplation cannot thus be limited simply to one definite period; and a further investigation is required in order to ascertain whether and in what way my second equation can be applied to such a case.

4. For this purpose we will carry a little further the already above intimated division of the total motion into two—the an-

gular motion of the radius vector, and the to-and-fro motion of the point within the radius vector.

The angular motion of the radius vector we will name *motion of rotation,* and understand by *period of rotation* the time during which the radius vector runs through the entire angular space 2π. The motion of the point within the radius vector we will name *radial motion of vibration,* or, briefly, *motion of vibration,* and choose the name *period of vibration* for the time during which a vibration to and fro takes place. The period of a rotation may be denoted by i, and the period of a vibration by i_1.

The motion of vibration can be treated as quite independent of the motion of rotation, if we introduce the centrifugal as a special force. Let θ denote the angle which the radius vector forms, at the time t, with a fixed right line in the plane of rotation, so that $\frac{d\theta}{dt}$ will denote the angular velocity of the radius vector; then the centrifugal force to which the rotation gives rise will be represented by the product

$$mr\left(\frac{d\theta}{dt}\right)^2.$$

Now, as for the motion of a point about a fixed centre of attraction the proposition is valid that the radius vector describes equal spaces in equal times, we have the equation

$$r^2\frac{d\theta}{dt}=c, \quad \ldots \ldots \quad (7)$$

in which c is a constant; and from this we immediately obtain, further,

$$mr\left(\frac{d\theta}{dt}\right)^2=mc^2\frac{1}{r^3}. \quad \ldots \ldots \quad (7a)$$

The centrifugal force represented by this expression we will now regard as a force of repulsion exerted from the centre, and to be added to the force represented by $F'(r)$, which is actually exerted from the centre. Then we obtain for the motion of vibration the following differential equation,

$$m\frac{d^2r}{dt^2}=-F'(r)+mc^2\frac{1}{r^3}, \quad \ldots \ldots \quad (8)$$

with the aid of which we can treat the vibration as a motion subsisting by itself. To this motion thus regarded, both my equations are found at once applicable.

Equation (2) gives, if we substitute r for x, and $-F'(r)+mc^2\frac{1}{r^3}$

for X,

$$\frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^2} = \tfrac{1}{2}\,\overline{r\mathrm{F}'(r)} - \frac{m}{2}c^2\,\overline{\frac{1}{r^2}}. \quad . \quad . \quad . \quad . \quad (9)$$

The second equation reads, in the form given under ($5c$) :—

$$\delta_1\overline{\mathrm{U}} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i.$$

In order to apply this equation, we have first to form the expression of the ergal U for the vibration-motion. In accordance with equation (8), we may put

$$\mathrm{U} = \int\left(\mathrm{F}'(r) - mc^2\,\frac{1}{r^3}\right)dr,$$

from which by integration, writing $\mathrm{F}(r)$ for the integral of $\mathrm{F}'(r)$, we get

$$\mathrm{U} = \mathrm{F}(r) + \frac{m}{2}c^2\,\frac{1}{r^2}.$$

This is such a function as was mentioned above, containing a quantity c which is constant during each motion, but may change its value on passing from one motion to another. This quantity must, when we form the variation $\delta_1\mathrm{U}$, be regarded as constant; and thus we obtain

$$\delta_1\mathrm{U} = \delta\mathrm{F}(r) + \frac{m}{2}c^2\delta\,\frac{1}{r^2}.$$

By introducing this expression into equation ($5c$), at the same time replacing v^2 by $\left(\frac{dr}{dt}\right)^2$ and using instead of i the symbol chosen for the period of a vibration, i_1, we transform that equation into

$$\delta\overline{\mathrm{F}(r)} + \frac{m}{2}c^2\delta\,\overline{\frac{1}{r^2}} = \frac{m}{2}\,\delta\overline{\left(\frac{dr}{dt}\right)^2} + m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (10)$$

From the two equations (9) and (10) we can eliminate the quantity $\overline{\left(\frac{dr}{dt}\right)^2}$. We will, however, for the present, only employ in the variation of $\overline{\left(\frac{dr}{dt}\right)^2}$ the expression given in (9). At the same time it is to be noticed that *this* is a complete variation, in which the change of the quantity c must also be considered. We thus get

$$\frac{m}{2}\,\delta\overline{\left(\frac{dr}{dt}\right)^2} = \tfrac{1}{2}\,\delta\overline{r\mathrm{F}'(r)} - \frac{m}{2}c^2\delta\,\overline{\frac{1}{r^2}} - m\,\overline{\frac{1}{r^2}}\,c\delta c. \quad . \quad (11)$$

This being put in (10) gives

$$\delta\overline{\mathrm{F}(r)}+\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}=\tfrac{1}{2}\delta\overline{r\mathrm{F}'(r)}-\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}-m\overline{\frac{1}{r^2}}c\delta c+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1,$$

or, differently arranged,

$$\delta\overline{\mathrm{F}(r)}-\tfrac{1}{2}\delta\overline{r\mathrm{F}'(r)}=-mc^2\delta\overline{\frac{1}{r^2}}-m\overline{\frac{1}{r^2}}c\delta c+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1,$$

for which we may write

$$\delta[\overline{\mathrm{F}(r)}-\tfrac{1}{2}\overline{r\mathrm{F}'(r)}]=-mc\delta\left(c\overline{\frac{1}{r^2}}\right)+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (12)$$

5. We will now turn to the motion of rotation.

In such a motion, in which the movable point need not have the same distance from the centre at the end as at the commencement of a rotation, the period of a rotation of the radius vector also need not be exactly the same for several successive rotations; yet at all events for a greater number of rotations we shall obtain a definite mean value of the period of a rotation. To this we will refer the symbol i. Now, pursuant to equation (7), we can write

$$d\theta=c\frac{1}{r^2}dt;$$

and when we integrate this equation for a whole number n of rotations, consequently between the limits $\theta=0$ and $\theta=n\,.\,2\pi$, there comes

$$n\,.\,2\pi=c\int_0^{ni}\frac{1}{r^2}dt=c\overline{\frac{1}{r^2}}ni,$$

and consequently

$$c\overline{\frac{1}{r^2}}=\frac{2\pi}{i}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

Putting this value of $c\overline{\frac{1}{r^2}}$ in equation (12), we obtain

$$\delta[\overline{\mathrm{F}(r)}-\tfrac{1}{2}\overline{r\mathrm{F}'(r)}]=-2\pi mc\delta\frac{1}{i}+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (14)$$

To this equation we can give a more symmetrical form; for the first term on the right-hand side can, in consideration of equation (13), be transformed thus:—

$$-2\pi mc\delta\frac{1}{i}=-mc^2\overline{\frac{1}{r^2}}i\delta\frac{1}{i}=mc^2\overline{\frac{1}{r^2}}\delta\log i.$$

Thereby the preceding equation is changed into

$$\delta[\overline{\mathrm{F}(r)}-\tfrac{1}{2}\overline{r\mathrm{F}'(r)}]=mc^2\overline{\frac{1}{r^2}}\delta\log i+m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log i_1. \quad . \quad (15)$$

The half of the factor $mc^2\overline{\frac{1}{r^2}}$ is the mean *vis viva* of the motion of rotation; for the component of velocity originated by the rotation is $r\frac{d\theta}{dt}$, and the part of the *vis viva* corresponding to this component is $\frac{m}{2}r^2\left(\frac{d\theta}{dt}\right)^2$, for which, according to (7), we can even put $\frac{m}{2}c^2\frac{1}{r^2}$. Just so the half of the factor $m\overline{\left(\frac{dr}{dt}\right)^2}$ is the mean *vis viva* of the motion of vibration. Accordingly the equation is symmetric in relation to the motions of rotation and vibration*. Since, according to (9), the sum of the two factors $mc^2\frac{1}{r^2}$ and $m\overline{\left(\frac{dr}{dt}\right)^2}$ is equal to $\overline{rF'(r)}$, we can even give to the preceding equation the following forms, more convenient for many applications:—

$$\delta[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}]=\overline{rF'(r)}\delta\log i-m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log\frac{i}{i_1}; \quad (16)$$

$$\delta[\overline{F(r)}-\tfrac{1}{2}\overline{rF'(r)}]=\overline{rF'(r)}\delta\log i_1+mc^2\overline{\frac{1}{r^2}}\delta\log\frac{i}{i_1}. \quad (17)$$

This equation, given in various forms under (14), (15), (16), and (17), is the expression of a new relation, universally valid for motions about a fixed centre of attraction.

When the central motion is of such a kind that a constant ratio subsists between the period of a rotation and the period of a vibration, the fraction $\frac{i}{i_1}$ is invariable, and therefore $\delta\log\frac{i}{i_1}=0$. Thereby the two preceding equations become accordant and are

* Pursuant to equation (13), we can also form the following equation:—

$$-\frac{m}{2}c^2\delta\overline{\frac{1}{r^2}}=\frac{m}{2}\delta\left(c^2\overline{\frac{1}{r^2}}\right)+mc^2\overline{\frac{1}{r^2}}\delta\log i.$$

Now, as the expression $mc^2\frac{1}{r^3}$ represents a force of attraction equal to the centrifugal force, we may regard the quantity $-\frac{m}{2}c^2\frac{1}{r^2}$, arising from the integration of that expression, as the ergal relative to the motion of rotation. Remembering further that $\frac{m}{2}c^2\frac{1}{r^2}$ represents the *vis viva* of the motion of rotation, we see that the preceding equation has precisely the same meaning for the motion of rotation as (10) has for the motion of vibration. The sum of the two equations, together with the consideration of (9), gives the preceding equation (15).

changed into

$$\delta[\overline{\mathrm{F}(r)}-\tfrac{1}{2}\overline{r\mathrm{F}'(r)}]=\overline{r\mathrm{F}'(r)}\,\delta\log i,$$

for which, as $\overline{r\mathrm{F}'(r)}=m\overline{v^2}$, we can write

$$\delta\overline{\mathrm{F}(r)}=\frac{m}{2}\,\delta\overline{v^2}+m\overline{v^2}\delta\log i.$$

This is the equation which holds for central motions in closed paths, and here appears as a special case of more general equations.

6. Having put the equations in such a manner that they are valid for any conceivable law of force, we will apply them to a special group of such laws, viz. to those according to which the force is *proportional to any power of the distance.* At the same time, however, we will exclude the minus first power, because in the integration it leads to logarithms, and consequently demands separate discussions, which would detract from the compendiousness of the analysis.

Accordingly, k and n denoting two constants, the latter of which is different from -1, we will put

$$\mathrm{F}'(r)=kr^n, \quad \ldots\ldots\ldots \quad (18)$$

whence results

$$\mathrm{F}(r)=\frac{k}{n+1}r^{n+1}. \quad \ldots\ldots \quad (19)$$

These formulæ we have to substitute for $\mathrm{F}(r)$ and $\mathrm{F}'(r)$ in the above equations. Equation (16) may be selected, which by the substitution is changed into

$$\frac{1-n}{2(n+1)}\,k\delta\overline{r^{n+1}}=k\overline{r^{n+1}}\delta\log i-m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log\frac{i}{i_1}. \quad . \quad (20)$$

For the sake of a more convenient expression, the quantity ρ shall now be introduced, with the signification

$$\rho^{n+1}=\overline{r^{n+1}}. \quad \ldots\ldots\ldots \quad (21)$$

This quantity can be immediately determined when the *energy* (sum of ergal and *vis viva*) is known, consequently when we know, for any position of the movable point, its velocity. That is to say, since the mean *vis viva* is equal to the virial, we have generally, if E denotes the energy,

$$\mathrm{E}=\overline{\mathrm{F}(r)}+\tfrac{1}{2}\overline{r\mathrm{F}'(r)},$$

and for our special law of force:—

$$\mathrm{E}=\frac{k}{n+1}\overline{r^{n+1}}+\frac{k}{2}\overline{r^{n+1}}=k\,\frac{n+3}{2(n+1)}\overline{r^{n+1}},$$

and consequently, employing the quantity ρ,

$$E=k\frac{n+3}{2(n+1)}\rho^{n+1},$$

from which it follows that

$$\rho=\left[\frac{2(n+1)}{k(n+3)}E\right]^{\frac{1}{n+1}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

After substituting, with the aid of this quantity ρ, for the mean value of a power, represented by $\overline{r^{n+1}}$, a simple power ρ^{n+1}, we can forthwith carry out the variation of it, and write

$$\delta\overline{r^{n+1}}=(n+1)\rho^n\delta\rho.$$

Accordingly, by the introduction of ρ, equation (20) changes into

$$\frac{1-n}{2}k\rho^n\delta\rho=k\rho^{n+1}\delta\log i-m\overline{\left(\frac{dr}{dt}\right)^2}\delta\log\frac{i}{i_1}. \quad . \quad . \quad (23)$$

Into this equation we will introduce a second simplifying quantity, p; for as the mean *vis viva* of the rotatory motion and the mean *vis viva* of the radial vibratory motion together make up the total mean *vis viva*, we can represent the two former as fractions of the latter, which fractions shall be denoted by p and $1-p$. Thus

$$\left.\begin{aligned}\frac{m}{2}c^2\overline{\frac{1}{r^2}}&=p\,\frac{k}{2}\rho^{n+1},\\ \frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^2}&=(1-p)\frac{k}{2}\rho^{n+1}.\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (24)$$

The value of p may vary between 0 and 1; and on it depends which out of all the forms possible with a given value of n the path takes. When $p=0$, the point moves in a right line to and from the centre; and when $p=1$, it moves in a circle round the centre. Between these two limits lie all the other possible forms.

By introducing p into equation (23) we obtain first

$$\frac{1-n}{2}k\rho^n\delta\rho=k\rho^{n+1}\delta\log i-(1-p)k\rho^{n+1}\delta\log\frac{i}{i_1};$$

and this equation, divided by $k\rho^{n+1}$, gives

$$\frac{1-n}{2}\delta\log\rho=\delta\log i-(1-p)\delta\log\frac{i}{i_1}. \quad . \quad . \quad . \quad . \quad (25)$$

As two of the terms of this equation are complete variations, the third term,

$$(1-p)\delta\log\frac{i}{i_1},$$

must likewise be such, whence it follows that p is a function of $\frac{i}{i_1}$ alone, or *vice versâ* $\frac{i}{i_1}$ is a function of p alone. Regarding $\frac{i}{i_1}$ as a function of p, we can from this function derive others; and we will introduce one, denoted by J, which shall be determined by the following equation:—

$$\log J = \int (1-p) \frac{d \log \frac{i}{i_1}}{dp} dp. \quad . \quad . \quad . \quad . \quad (26)$$

Then is

$$(1-p)\delta \log \frac{i}{i_1} = \delta \log J;$$

and therefore equation (25) changes into

$$\frac{1-n}{2} \delta \log \rho = \delta \log i - \delta \log J. \quad . \quad . \quad . \quad (27)$$

By transposition and integration of this equation we obtain

$$\log i = \frac{1-n}{2} \log \rho + \log J + \text{const.}$$

It is immaterial which value we attribute to the integration-constant, since, in accordance with (26), we can suppose any additive constant we please to be likewise contained in $\log J$. The value most suitable for what follows is $\log 2\pi\sqrt{\frac{m}{k}}$. If we put this quantity in the place of the constant, and then combine the three logarithms of the right-hand side, we get

$$\log i = \log\left(2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}} J\right);$$

and from this we obtain, for the rotation-period i, the following simple expression:—

$$i = 2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}} J. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

We can deduce a corresponding expression for the oscillation-period i_1. Equation (25), namely, can also be written in the following form:—

$$\frac{1-n}{2} \delta \log \rho = \delta \log i_1 + p\delta \log \frac{i}{i_1}.$$

Introducing into this the function J_1, which is determined by

the following equation,

$$\log J_1 = -\int p \frac{d \log \frac{i}{i_1}}{dp} dp, \quad . \quad . \quad . \quad . \quad . \quad (29)$$

the result is similar to the preceding:—

$$i_1 = 2\pi \sqrt{\frac{m}{k}} \rho^{\frac{1-n}{1}} J_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (30)$$

Between the functions J and J_1, occurring in these two expressions, there subsists, according to equations (26) and (29), the following relation:—

$$p \frac{d \log J}{dp} = (p-1) \frac{d \log J_1}{dp}. \quad . \quad . \quad . \quad . \quad (31)$$

As, with the aid of this equation, one of the two functions can be deduced from the other, we can say that, in the two expressions of i and i_1, only one undetermined function of p occurs.

7. In equations (28) and (30), the periods i and i_1 are expressed by the quantities ρ and p. According to equation (22), ρ is a simple function of the energy E, which remains invariable during the whole motion, and hence can be taken as known. It is otherwise with the quantity p. It is true this has a simple signification (the mean *vis viva* of the motion of rotation as a fraction of the total mean *vis viva*); but its value cannot be stated so simply, because for the calculation of the mean value of a variable quantity the whole course of the motion must be taken into consideration. Hence it is advisable to introduce instead of p another quantity the value of which is obtained immediately from the data usually employed for the determination of the motion.

These are the energy E and the already discussed quantity c, the half of which represents the area described by the radius vector in the unit of time, and which, as well as the energy, remains constant during the whole motion. We will now bring in a quantity q determined by the following equation,

$$q = m^{\frac{1}{2}} k^{\frac{1}{n+1}} c \left[\frac{2(n+1)}{n+3} E\right]^{\frac{n+3}{2(n+1)}}, \quad . \quad . \quad . \quad (32)$$

and which, consequently, can be calculated in a very simple manner from E and c.

In order to find the connexion of this new quantity q with p, we can first, with the aid of equation (22), transform the expression of q into the following:—

$$q = \sqrt{\frac{m}{k}} \frac{c}{\rho^{\frac{n+3}{2}}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (33)$$

Further, equations (13) and (24) hold good—

$$c\overline{\frac{1}{r^2}}=\frac{2\pi}{i},$$

$$\frac{m}{2}c^2\overline{\frac{1}{r^2}}=p\frac{k}{2}\rho^{n+1},$$

from which, by elimination of $\overline{\frac{1}{r^2}}$ results

$$i=2\pi\frac{m}{k}\frac{c}{p\rho^{n+1}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (34)$$

If herein for i we put its value from (28), we obtain

$$J=\sqrt{\frac{m}{k}}\frac{c}{p\rho^{\frac{n+3}{2}}}; \quad . \quad . \quad . \quad . \quad . \quad (35)$$

and the combination of this with equation (33) gives

$$q=pJ. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (36)$$

Consequently q stands in a very simple relation to p.

Its behaviour is also very similar to that of p. It can likewise vary only between the limits 0 and 1, and takes these boundary values simultaneously with p. When $c=0$, then, according to (32), is q also $=0$; and consequently the *vis viva* of the rotation-motion also, and therefore the quantity p, has the value zero. If, now, c is increasing, p and q increase at the same time. When p has reached the value 1 the path has become circular. For this case we have, in the differential equation

$$m\frac{d^2r}{dt^2}=-kr^n+mc^2\frac{1}{r^3}$$

for the oscillatory motion, to put the differential coefficient $\frac{d^2r}{dt^2}$ equal to zero; and the equation thereby arising can be brought into the following form:—

$$\frac{m}{k}\frac{c^2}{r^{n+3}}=1.$$

Further, where r is constant, we may regard r and ρ as signifying the same thing, and hence the preceding equation can also be written

$$\frac{m}{k}\frac{c^2}{\rho^{n+3}}=1;$$

and this gives, in accordance with (33), for q the value 1.

Since, according to (36), q is represented by the product pJ, in which J is a function of p only, q itself is likewise a function of p alone, and accordingly p can reciprocally be regarded as a function of q only. Thence it follows, further, that J and J_1, which

we have hitherto written as functions of p, can just as well be regarded as functions of q; and for this we have only to transform the relation between J and J_1 expressed in (31) so that it shall contain q in the place of p.

Equation (31) can be written in the following form:—

$$\frac{d\log J_1}{dp}=p\frac{d}{dp}\left(\log\frac{J_1}{J}\right). \quad . \quad . \quad . \quad . \quad (37)$$

Differentiation of the logarithms gives

$$\frac{1}{J_1}\frac{dJ_1}{dp}=p\frac{J}{J_1}\frac{d}{dp}\left(\frac{J_1}{J}\right).$$

Here J_1 can be taken away from both denominators; and, according to (36), q can be substituted for pJ. Further, we can transform the two differential coefficients according to p into such according to q by employing the general equation

$$\frac{dZ}{dp}=\frac{dZ}{dq}\frac{dq}{dp},$$

wherein the differential coefficient $\frac{dq}{dp}$, which occurs on both sides, can be omitted. We thus obtain

$$\frac{dJ_1}{dq}=q\frac{d}{dq}\left(\frac{J_1}{J}\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad (38)$$

This is the relation sought between J and J_1. It is seen that the new equation, in relation to J_1, $\frac{J_1}{J}$, and q, has the same form as (37) in relation to $\log J_1$, $\log\frac{J_1}{J}$, and p.

8. In the preceding, for central motions in which the attractive force is proportional to any power of the distance, a series of formulæ is given which represent the times corresponding to the motion-periods, and various mean values, as functions of two easily determinable quantities. As these formulæ are scattered among the equations applied to their derivation, it will be advisable, for the sake of a more convenient review of them, to briefly recapitulate them in juxtaposition.

E denoting the energy of the moving point, and c twice the value of the area described by the radius vector during the unit of time, we first form the following two quantities:—

$$\rho=\left[\frac{2(n+1)}{k(n+3)}E\right]^{\frac{1}{n+1}},$$

$$q=m^{\frac{1}{2}}k^{\frac{1}{n+1}}c\left[\frac{2(n+1)}{n+3}E\right]^{-\frac{n+3}{2(n+1)}}=\sqrt{\frac{m}{k}}\frac{c}{\rho^{\frac{n+3}{2}}}.$$

We next introduce two functions of q, denoted by J and J_1, which are connected by the following equation:—

$$\frac{dJ_1}{dq}=q\frac{d}{dq}\left(\frac{J_1}{J}\right).$$

Then for the undermentioned quantities the equations written after them are valid:—

(1) The mean ergal:

$$\frac{k}{n+1}\overline{r^{n+1}}=\frac{k}{n+1}\rho^{n+1}.$$

(2) The mean *vis viva*:

$$\frac{m}{2}\overline{v^2}=\frac{k}{2}\rho^{n+1}.$$

(3) The mean *vis viva* of the rotatory motion (according to equation (24), replacing p therein by $\frac{q}{J}$):

$$\frac{m}{2}c^2\overline{\frac{1}{r^2}}=\frac{k}{2}\rho^{n+1}\frac{q}{J}.$$

(4) The mean *vis viva* of the radial oscillatory motion:

$$\frac{m}{2}\overline{\left(\frac{dr}{dt}\right)^2}=\frac{k}{2}\rho^{\ +1}\left(1-\frac{q}{J}\right).$$

(5) The period of a rotation:

$$i=2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}J.$$

(6) The period of a vibration:

$$i_1=2\pi\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}J_1.$$

Besides the above quantities, others can easily be expressed after the above developments—for example, the mean ergal of the rotatory motion and of the vibratory motion; meanwhile the preceding expressions may suffice.

9. In all these expressions, only one undetermined function of q occurs, since the two denoted by J and J_1 can, through the relation subsisting between them, be reduced to one. To determine this function also was not originally included in the plan of my investigation, as I only wished to deduce those consequences which result immediately from my new mechanical equations; but after I had produced the above formulæ it seemed to me advisable, for the sake of completeness, to undertake at

least an approximative determination of that function. I have therefore developed the function J_1 in a series, and calculated some of the terms of that series.

From the above-mentioned differential equation

$$m\frac{d^2r}{dt^2}=-kr^n+mc^2\frac{1}{r^3},$$

we obtain by the first integration:—

$$\frac{m}{2}\left(\frac{dr}{dt}\right)^2=-\frac{k}{n+1}r^{n+1}-\frac{m}{2}c^2\frac{1}{r^2}+\text{E},$$

wherein E, as before, denotes the energy of the motion. This gives, for the determination of the time, the differential equation

$$dt=\frac{dr}{\sqrt{\frac{2}{m}\text{E}-c^2\frac{1}{r^2}-\frac{k}{m}\frac{2}{n+1}r^{n+1}}};$$

or, multiplying both numerator and denominator by r,

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{-c^2+\frac{2}{m}\text{E}r^2-\frac{k}{m}\frac{2}{n+1}r^{n+3}}}. \quad . \quad . \quad (39)$$

Into this we will introduce the constants ρ and q instead of E and c, putting, according to equations (22) and (33),

$$\text{E}=k\frac{n+3}{2(n+1)}\rho^{n+1} \text{ and } c^2=\frac{k}{m}q^2\rho^{n+3}.$$

We then get

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{-\frac{k}{m}q^2\rho^{n+3}+\frac{k}{m}\frac{n+3}{n+1}\rho^{n+1}r^2-\frac{k}{m}\frac{2}{n+1}r^{n+3}}}.$$

If we take away the factor $\frac{k}{m}\rho^{n+3}$ from the root-sign, we obtain

$$dt=\frac{\frac{1}{2}d(r^2)}{\sqrt{\frac{k}{m}}\rho^{\frac{n+3}{2}}\sqrt{-q^2+\frac{n+3}{n+1}\left(\frac{r}{\rho}\right)^2-\frac{2}{n+1}\left(\frac{r}{\rho}\right)^{n+3}}};$$

or, otherwise written,

$$dt=\frac{1}{2}\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\frac{d\left(\frac{r^2}{\rho^2}\right)}{\sqrt{-q^2+\frac{n+3}{n+1}\left(\frac{r}{\rho}\right)^2-\frac{2}{n+1}\left(\frac{r}{\rho}\right)^{n+3}}}. \quad . \quad (40)$$

Herein, for convenience we will put, instead of the fraction $\frac{r}{\rho}$,

$$dt=\tfrac{1}{2}\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}\quad . \quad . \quad . \quad (41)$$

This equation must be integrated in order to obtain the vibration-period. Therein, as limits, two values of x are to be taken for which the expression under the root-sign, and accordingly the radial velocity $\frac{dr}{dt}$, becomes $=0$. These values may be denoted by x_0 and x_1. The integral from one limit to the other gives the time needed by the movable point in order to arrive at the highest from the lowest value; but as we understand by the vibration-period the time employed by the point in attaining from the lowest to the highest value and then again to the lowest, the double of the above-mentioned must be taken. We thus obtain

$$i_1=\sqrt{\frac{m}{k}}\rho^{\frac{1-n}{2}}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}. \quad (42)$$

Comparing this expression for i_1 with that given in (30), we obtain for the function J_1 the equation

$$J_1=\frac{1}{2\pi}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{-q^2+\frac{n+3}{n+1}x^2-\frac{2}{n+1}x^{n+3}}}\quad . \quad . \quad (43)$$

10. In order to effect this integration, we will first write the equation thus:—

$$J_1=\frac{1}{2\pi}\int_{x=x_0}^{x=x_1}\frac{d(x^2)}{\sqrt{1-q^2-\left(1-\frac{n+3}{n+1}x^2+\frac{2}{n+1}x^{n+3}\right)}}. \quad (44)$$

Into this we will introduce a new variable, z, determined by the following equation:—

$$1-\frac{n+3}{n+1}x^2+\frac{2}{n+1}x^{n+3}=z^2. \quad . \quad . \quad . \quad (45)$$

If we then imagine the quantity x^2 developed in a series according to ascending powers of z, putting

$$x^2=a+a_1z+a_2z^2+a_3z^3+\&c., \quad . \quad . \quad . \quad . \quad (46$$

and so determine the coefficients of this series that they satisfy the preceding equation, we obtain the following values, in which,

further, for abbreviation, μ with the signification

$$\mu=\frac{(n+2)(n-1)}{n+3} \quad . \quad . \quad . \quad . \quad . \quad (47)$$

is introduced:—

$$\left.\begin{aligned}
a &=1,\\
a_1 &=\pm\frac{2}{\sqrt{n+3}},\\
a_2 &=-\frac{n-1}{n+3}\cdot\frac{1}{3},\\
a_3 &=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{1}{2\,.\,3^2},\\
a_4 &=-\mu\frac{n+5}{n+3}\cdot\frac{1}{3^3\,.\,5},\\
a_5 &=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{2^3\,.\,3+\mu}{2^4\,.\,3^3\,.\,5},\\
a_6 &=-\mu\frac{n+5}{n+3}\cdot\frac{2\,.\,3^2-\mu}{3^5\,.\,5\,.\,7},\\
a_7 &=\pm\frac{\mu}{\sqrt{n+3}}\cdot\frac{2^4\,.\,3^3\,.\,5^2-2^6\,.\,3^2\mu-139\mu^2}{2^6\,.\,3^5\,.\,5^2\,.\,7}.
\end{aligned}\right\} \quad . \quad (48)$$

By the employment of the new variable, z, equation (44) is changed into the following, in which the limits of the integration are definitely given:—

$$J_1=\frac{1}{2\pi}\int_{z=-\sqrt{1-q^2}}^{z=\sqrt{1-q^2}}\frac{d(a+a_1z+a_2z^2+a_3z^3+\&c.)}{\sqrt{1-q^2-z^2}} \quad . \quad . \quad (49)$$

The integral herein signified falls, according to the terms of the series, into an infinite number of integrals, the values of which can easily be given. Every term that contains an even power of z gives as an integral the value zero. For the terms with odd powers the following general equation (in which ν is to be an uneven integer) holds good:—

$$\int_{z=-\sqrt{1-q^2}}^{z=\sqrt{1-q^2}}\frac{d(z^\nu)}{\sqrt{1-q^2-z^2}}=\frac{1\,.\,3\,.\,5\,.\,7\ldots\nu}{1\,.\,2\,.\,4\,.\,6\ldots\nu-1}(1-q^2)^{\frac{\nu-1}{2}}\pi.$$

If we apply this formula to the odd terms of the preceding equation, we obtain first

$$J_1=\frac{1}{2}\left[a_1+\frac{3}{2}a_3(1-q^2)+\frac{3\,.\,5}{2\,.\,4}a_5(1-q^2)^2+\&c.\right] \quad . \quad . \quad (50)$$

In this equation, for the coefficients a, a_3, &c. we have to put

their values derived from (48), taking into account only the upper of the two signs which stand before the roots, as the lower sign would give a negative period. Thereby we obtain

$$J_1 = \frac{1}{\sqrt{n+3}}\Big[1 + \frac{\mu}{2^3.3}(1-q^2) + \mu\frac{2^3.3+\mu}{2^8.3^2}(1-q^2)^2$$

$$+\mu\frac{2^4.3^3.5^2-2^6.3^2\mu-139\mu^2}{2^{11}.3^5.5}(1-q^2)^3 + \&c.\Big]. \quad . \quad (51)$$

Hereby the quantity J_1 as a function of q is so far determined that, for values of q not deviating too much from unity, it can be calculated with tolerable approximateness.

From this series, with the aid of (38), the corresponding series for J can be derived.

11. This derivation and other calculations become somewhat easier when the series is developed according to ascending powers of $1-q$. We will represent the latter difference by a special letter, putting

$$s = 1 - q. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (52)$$

Then

$$1-q^2 = -(1-s)^2 = 2s - s^2;$$

and the insertion of this value changes the above series into

$$J_1 = \frac{1}{\sqrt{n+3}}\Big[1 + \frac{\mu}{2^2.3}s + \frac{\mu^2}{2^6.3^2}s^2$$

$$-\mu\frac{2^4.3^3.5+2^2.3^2.31\mu+139\mu^2}{2^8.3^5.5}s^3 + \&c.\Big]. \quad (53)$$

In order from this to calculate J, we can write equation (38) in the following form:—

$$\frac{d}{ds}\left(\frac{J_1}{J}\right) = \frac{1}{1-s}\frac{dJ_1}{ds}. \quad . \quad . \quad . \quad . \quad . \quad (54)$$

Putting herein for J_1 the preceding expression, we obtain first the differential coefficient of $\frac{J_1}{J}$, and then, by integration, $\frac{J_1}{J}$ itself. The added integration-constants can be readily determined, because for $s=0$, consequently for the circular motion, the rotation-period i, and accordingly the value of the function J, can be directly determined. It follows, namely, that for this case J is to be put $=1$, whence $\frac{J_1}{J} = \frac{1}{\sqrt{n+3}}$. Taking into ac-

count this value gives for $\frac{J_1}{J}$ the following series:—

$$\frac{J_1}{J}=\frac{1}{\sqrt{n+3}}\Big[1+\frac{\mu}{2^2.3}s+\mu\frac{2^3.3+\mu}{2^6.3^2}s^2$$
$$+\mu\frac{2^4.3^4.5-2^2.3^3.7\mu-139\mu^2}{2^8.3^5.5}s^3+\&c.\Big]. \quad (55)$$

As the fraction $\frac{J_1}{J}$ is equal to the fraction $\frac{i_1}{i}$, the preceding expression represents the ratio between the vibration-period and the rotation-period.

We might now, in order to determine J, simply divide equation (53) by equation (55); but then we should only obtain J developed as far as the third power, while the term containing the fourth power is obtainable; for if we write equation (54) in the form

$$\frac{d}{ds}\left(\frac{J_1}{J}-J_1\right)=s\frac{d}{ds}\left(\frac{J_1}{J}\right), \quad . \quad . \quad . \quad . \quad . \quad (56)$$

we can from this, on account of the factor s on the right-hand side, determine to the fourth power of s the difference $\frac{J_1}{J}-J_1$ (which does not contain a constant term, but begins at once with the second power of s), although $\frac{J_1}{J}$ is known only to the third power. If we then form the identical equation

$$J=1-\frac{\frac{J_1}{J}-J_1}{\frac{J_1}{J}},$$

and employ, on the right-hand side of it, instead of the difference which forms the numerator of the second term, the expression developed to the fourth power, we get

$$J=1-\frac{\mu}{2^3.3}s^2+\mu\frac{-2^2.3+\mu}{2^4.3^3}s^3$$
$$+\mu\frac{-2^4.3^4.5+2^2.3.7.29\mu+89\mu^2}{2^{10}.3^4.5}s^4+\&c. \quad (57)$$

12. We have therefore obtained for the functions J and J_1 expressions from which their values can be approximately determined, and indeed with an accuracy proportional to the smallness of s, or inversely as the deviation of the path from the circular form. They afford, in many a relation, a ready insight into the behaviour of these functions.

The factor preceding the series in the expression for J_1, viz. $\frac{1}{\sqrt{n+3}}$, which for $n=-3$ is infinitely large, and for values of n below -3 becomes imaginary, shows very clearly that, for attractive forces proportional to a negative power of the distance, the minus third power constitutes the limit to which stationary motions are in general possible.

Further, it is characteristic of both expressions that all the terms which contain s have the common factor μ. From this it follows that, when $\mu=0$, both expressions become independent of the quantity s, equations (53) and (57) changing into

$$J_1=\frac{1}{\sqrt{n+3}} \text{ and } J=1.$$

Now, since according to (47)

$$\mu=\frac{(n+2)(n-1)}{n+3},$$

this simple behaviour occurs in the two cases in which n has the values -2 and 1.

These two values of n are the only ones with which the motions universally (that is, for all values of s) take place in closed paths. That is to say, this can only be the case when the ratio between the vibration-period and the rotation-period is invariable, and hence $\frac{i_1}{i}$, which is identical with $\frac{J_1}{J}$, must be independent of s. Thence it follows further that in the series under (55) the coefficients of all the powers of s, from the first onward, must be $=0$, so that the constant part alone remains—which only happens when $\mu=0$ and consequently n is either $=-2$ or $=1$.

For $\mu=0$, the fraction $\frac{i_1}{i}$ is represented by the formula $\frac{1}{\sqrt{n+3}}$, which, according as $n=-2$ or $n=1$, takes the value 1 or $\frac{1}{2}$. By this is expressed that, in the former case, during a rotation one radial vibration takes place, so that the radius vector has one maximum and one minimum—while in the latter case there are two radial vibrations, so that the radius vector has two maxima and two minima.

All the preceding considerations relate to the motions of a material point about a fixed centre. Considerations altogether similar might be instituted in relation to the motions of two material points about each other, and would lead to corresponding results. In my previous memoirs I have carried out this extension for the case in which the motions take place in closed paths; and with motions in paths not closed the extension can be made in substantially the same manner.

II. *On the Composition and Origin of the Waters of a Salt Spring in Huel Seton Mine, with a Chemical and Microscopical Examination of certain Rocks in its vicinity.* *By* J. ARTHUR PHILLIPS. *M.Inst.C.E., F.G.S., F.C.S., &c.**

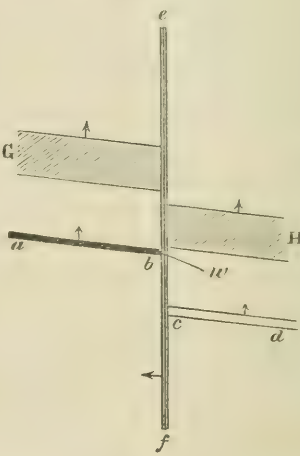
[With a Plate.]

HUEL SETON Copper-mine is situated about one mile north-east of the town of Camborne, Cornwall, and is distant from the sea, on the north coast, a little more than three miles. Its position will be readily understood by the aid of the accompanying skeleton map (Plate I.) of the district, traced from that of the Geological Survey.

The workings of Huel Seton are entirely in "killas," or clay-slate; and the saline waters issue at the rate of 50 gallons per minute, and at a temperature of 92° F., from the eastern fore-breast of the 160-fathom level†. This has intersected a fault or cross-course, which can be traced in a northerly direction to the sea. The temperature of the level from the end of which the water issues, like that of the water itself, is 92° F.

The lode *a b*, *c d*, fig. 1, which is not well defined, and is frequently a mere narrow fissure in the clay-slate, has been driven on to its point of intersection *b* with the cross-course *e f*, and is believed to have been thrown in a southerly direction from *b* to *c*. A dyke of porphyry, G H, 40 feet in width, courses parallel with the lode at a distance of a few fathoms to the north; but its intersection with the cross-course not having been seen, it is not known whether any throw or dislocation actually takes place at this point, as

Fig. 1.



* Communicated by the Author, having been read before the Royal Society, January 30, 1873.

† My attention was first directed to the discovery of this spring by Mr. W. J. Henwood, F.R.S., of Penzance, who forwarded me in June last a cutting from a local newspaper headed "A Salt Spring in Huel Seton Mine." This notice states that the water had been sent to Mr. S. T. Rowe, analyst to the Truro Agricultural Association, and that an analysis had been returned "showing it to be highly impregnated with salt, salts of lime, and other chemical matter to the extent of 1072 grains to the imperial gallon." It then goes on to say, "very little iron, no copper, and no

shown in the sketch; the direction of the dip is in each case indicated by an arrow. The water which issues from the point *w* was collected and brought to the surface in carefully cleaned stoneware jars.

The following results, in grammes per litre and grains per gallon, were obtained by analysis.

Mineral Water from Huel Seton.

Solid matter 14·3658 grammes per litre, or 1005·61 grains per gallon. Sp. gr. =1·0123.

Analytical Results.

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Carbonic acid......	·0795	·0786	5·56	5·50
Sulphuric acid ...	·0178	·0177	1·25	1·24
Silica	·0270	·0280	1·89	1·96
Chlorine...........	9·1728	9·1662	642·10	641·63
Bromine............	trace	trace	trace	trace
Alumina...........	·3456	·3460	24·19	24·22
Ferric oxide	·0031	·0033	·22	·23
Manganese.........	trace	trace	trace	trace
Copper	minute trace	minute trace	minute trace	minute trace
Lime	3·4795	3·4963	243·56	244·74
Magnesia	·0721	·0710	5·05	4·97
Alkaline chlorides.	6·4920	6·4626	454·44	452·38
Potassium	·0832	·0835	5·82	5·84
Cæsium*............	trace	trace	trace	trace
Sodium	2·2977	2·2885	160·84	160·19
Lithium	·0805	·0794	5·63	5·56
Ammonia	trace	trace	trace	trace
Nitric acid	trace	trace	trace	trace

The foregoing results may be thus tabulated† :—

gas, beyond carbonic acid, was found by the analyst." The presence of lithium is not mentioned. A few days after the receipt of the cutting above referred to, another copy was received from Mr. R. Hunt, F.R.S., and a third from Mr. J. H. Collins, F.G.S., of Falmouth. I have also much pleasure in acknowledging my obligations to Captain R. Williams, the Manager of the Mines, for the facilities afforded by him for the collection of the water for analysis.

* The amount of cæsium appears to be very small. On adding chloride of platinum to a rather dilute solution of the alkaline chlorides obtained from this water, a slight yellow precipitate was deposited; this, after re-solution and the removal of the platinum by sulphuretted hydrogen, afforded by the spectroscope indications of the presence of cæsium.

† As the state of combination in which the various substances present in mineral waters exist cannot be accurately determined, the system of grouping adopted in the Table must to some extent be regarded as arbitrary.

	Grammes per litre.		Grains per gallon.	
	I.	II.	I.	II.
Calcium carbonate............	·0921	·1011	6·45	7·08
Ferrous carbonate...........	·0045	·0047	·31	·33
Manganous carbonate	trace	trace	trace	trace
Calcium sulphate	·0303	·0301	2·12	2·11
Cupric chloride	minute trace	minute trace	minute trace	minute trace
Calcium chloride	6·7697	6·7934	473·88	475·54
Magnesium chloride	·1712	·1686	11·98	11·80
Aluminium chloride	·9003	·9013	63·02	63·09
Potassium chloride	·0919	·0900	6·43	6·30
Cæsium chloride	trace	trace	trace	trace
Sodium chloride	5·8442	5·8210	409·09	407·47
Lithium chloride	·4888	·4820	34·22	33·74
Potassium bromide	trace	trace	trace	trace
Potassium silicate (K^2SiO^3).	·0693	·0719	4·85	5·03
Ammonia	trace	trace	trace	trace
Nitric acid......................	trace	trace	trace	trace
Total found by addition ...	14·4623	14·4641	1012·35	1012·49
Total found directly*	14·3658	......	1005·61	
Free carbonic acid............	·0373	·0323	2·61	2·26

The nature and amount of the various substances in solution in the saline waters having been determined, it was considered desirable to ascertain by analysis the composition of the principal rocks occurring in the neighbourhood of the spring. It was hoped that in this way information might be obtained respecting the sources from which some of the substances taken up by the water had been derived. It was further thought that, by comparing the composition of the rock in the immediate vicinity of the cross-course through which the heated waters issue with that of the clay-slate further removed from its influences, it might be possible to ascertain some of the effects produced by their action. In the case of each of the rocks analyzed, numerous thin sections were cut and subjected to careful microscopical examination.

"*Elvan*," sp. gr. = 2·64.—The rock constituting the dyke G H, fig. 1, is exceedingly hard and compact, presenting the appearance of a crystalline greyish matrix, enclosing numerous well-defined crystals of flesh-coloured orthoclase; a specimen of this rock afforded by analysis the following results:—

* The difference between the amount of total solid contents found directly and that obtained by the addition of constituents, is doubtless partly due to the partial decomposition of aluminium and magnesium chlorides at the temperature (160° C.) at which the drying of the residue was effected.

	I.	II.
Water*	1·53	1·56
Silica	70·76	70·85
Alumina	16·78	16·86
Ferrous oxide	2·44	2·50
Manganous oxide	1·24	1·18
Sulphur	trace	trace
Lime	1·23	1·21
Magnesia	1·55	1·52
Potassa	3·08	3·20
Soda	·85	·92
	99·46	99·80

Under the microscope this rock is seen to consist of a felsitic base containing crystals of orthoclase. The base is composed of highly altered felspar, pseudomorphs, apparently after hornblende, and numerous sphærulites exhibiting a radial structure. These sphærulitic bodies are not terminated by sharp outlines, but become gradually blended with the surrounding base; their radial structure, however, is perfectly defined, particularly when examined by polarized light. There is a green flocculent substance disseminated throughout the base; and many small patches of quartz are observed, which, by polarized light, appear to break up into granules. There are also prisms of a green colour, which are apparently altered hornblende; in a few cases one portion of a crystal is seen to be composed of some hornblendic mineral, while the remainder has been replaced by felspar. In some of the sections examined minute spots and microscopic crystals of iron pyrites were observed.

This rock would be generally described as a porphyry; but Mr. S. Allport, who at my request had the kindness to cut and examine a section of it, considers its composition would be better indicated by the name Porphyritic Felsite. It has, however, undergone a considerable amount of alteration; and although the larger crystals of orthoclase are for the most part unchanged, the smaller ones are almost without exception pseudomorphs.

Granite from Carn Brea, sp. gr. = 2·64.—The Carn Brea Hill, about a mile south-east of Huel Seton, is composed of a rather coarse-grained granite, in which the proportion of quartz is generally large; the felspar sometimes occurs in crystals of considerable size. It contains two varieties of mica, the one black (lepidomelane?), and the other white or slightly tinged with pink; the latter appears to be a variety of lepidolite.

The chemical composition of this granite was found to be as follows:—

* Of which ·19 was lost in the water-bath.

	I.	II.
Water*	1·20	1·26
Silica	74·62	74·76
Alumina	16·29	16·13
Ferrous oxide . . .	1·18	1·15
Ferric oxide . . .	trace	trace
Manganous oxide . .	·55	·62
Lime	·30	·26
Magnesia	·50	·46
Potassa	3·54	3·74
Soda	1·24	1·13
Lithia	·11	·09
	99·53	99·60

A microscopical examination of this rock does not afford much information not to be obtained by a close inspection of hand-specimens. A large proportion of the felspar is seen to be monoclinic; but triclinic felspar is also present in considerable quantity. The outlines of the crystals are not in all cases sharply defined, and they are sometimes rendered slightly cloudy by some flocculent chloritic mineral. Two varieties of mica are distinctly visible, and occur in both quartz and felspar, together with a little tourmaline. The quartz is much fissured, the sides of the cracks being apparently coated by ferric oxide; it contains numerous fluid-cavities enclosing bubbles, which in some of the smaller ones are observed to be in constant motion.

Clay-slate &c.—In those portions of the mine situated at a distance from the cross-course, the "killas" is a very hard clay-slate of a grey colour. Cleavage is in the majority of cases to some extent obliterated; no trace of crystalline structure can be detected; and the rock, in addition to being traversed by numerous strings of white quartz, is thickly studded with minute spots and crystals of iron pyrites. Sp. gr. =2·73.

In the immediate vicinity of the fault, which appears to furnish a channel for the passage of the saline waters, the slate has become much altered, is of a dark green colour, consists to a great extent of minute acicular crystals, with their longer axes parallel to the planes of cleavage, and in many cases closely resembles some varieties of serpentine. Crystals of chlorite are found in the joints of cleavage. It also exhibits many of those highly polished surfaces so frequently met with in serpentinous rocks. Sp. gr. =2·69.

By analysis these rocks were respectively found to have the following compositions:—

* Of which ·34 was lost in the water-bath.

	Unaltered clay-slate.		Rock in vicinity of cross-course.	
	I.	II.	I.	II.
Water*	3·13 *a*	3·10	6·87 *b*	6·84
Silica	67·78	67·87	45·85	45·97
Alumina	9·60	9·52	16·60	16·78
Ferrous oxide	5·09	4·96	10·59	10·72
Ferric oxide	trace	trace	trace	trace
Manganous oxide	1·27	1·14	1·16	1·12
Lime	2·62	2·54	2·68	2·68
Magnesia	3·48	3·37	6·94	6·87
Potassa	2·45	2·30	·92	·78
Lithia	......	......	trace	trace
Soda	4·46	4·32	5·64	5·53
Sulphur	·36 } = ·68 FeS^2	·32 } = ·60 FeS^2	·78 } = ·146 FeS^2	·80 } = ·150 FeS^2
Iron	·32	·28	·68	·70
Sulphuric acid	......	......	·76	·74
Chlorine	......	......	trace	trace
	100·56	99·72	99·47	99·53

Thin sections of the clay-slate situated at a distance from the cross-course are seen under the microscope to consist of bands of transparent granular quartz alternating with layers of similar quartz, through which a transparent, dichroic, pinkish-brown mineral is thickly disseminated in the form of imperfectly defined elongated crystals. The outline of these, of which the dimensions of the largest are $\frac{3}{1000} \times \frac{1}{1000}$ of an inch, is so irregular, and they are so crowded and interlaced, that it is impossible to determine their crystalline form; they are, however, believed to be hornblende.

When examined with an inch objective, the mass, in addition to numerous crystalline bodies, appears to be full of a brown dust, arranged in approximately parallel bands, which much diminish the transparency of those portions of the sections in which it most plentifully occurs. By the aid of a power magnifying 510 diameters, this dust is resolved into crystalline tufts of a dark colour and hornblendic appearance. Dendritic patches and crystals of iron pyrites are disseminated throughout the rock; and some of the veins by which it is traversed enclose well-defined crystals of both chlorite and hornblende.

The altered rock in the immediate vicinity of the cross-course is seen to be composed of closely matted prisms of brown hornblende enclosing pale green crystals of actinolite. It also contains a few crystals of quartz, and iron pyrites in the form of minute cubes.

* *a* lost ·94 per cent. in the water-bath, and *b* 1·55 per cent.

Source whence the mineral waters of Huel Seton are probably derived.—Before attempting to account for the presence of the mineral constituents found in these waters, it will be necessary to consider the bearing and importance of the following facts:—

(*a*) The average elevation of the surface of the mine is about 300 ft. above the sea; and the 160-fathom level, being 960 feet below the adit or drainage-tunnel and 1080 feet from the surface, is consequently much beneath low-water mark.

(*b*) The cross-course, shown in the accompanying geological map, may, as has been already stated, be traced for a distance of three miles to the coast, and apparently forms the channel through which the saline waters effect an entrance into the workings.

(*c*) The water contains a very large proportion of chloride of sodium.

(*d*) Similar springs of hot saline water were met with, below the level of the sea, in the neighbouring mines of North Roskear and North Crofty, both situated on the same cross-course. These waters have not been analyzed.

(*e*) A hot spring yielding waters possessing the same general characteristics as those from Huel Seton, formerly issued at the Huel Clifford Mines in the 230-fathom level, or at a depth of 1320 feet below the sea. It will be seen, by referring to the Map, that in this case a well-defined cross-course can be continuously traced in a north-westerly direction from the immediate vicinity of the spring to the sea at Tobban Cove*.

* These mines are now abandoned and consequently flooded. The waters issued at a temperature of 125° F., and at the rate of 150 gallons per minute. The late Dr. W. A. Miller, who analyzed them in 1864, obtained the following results:—

Sp. gr. = 1·007. The saline constituents were found by evaporation to amount to 646·1 grains per imperial gallon, consisting of:—

Chloride of lithium	26·05
Chloride of potassium with a little chloride of cæsium	14·84
Chloride of sodium	363·61
Chloride of magnesium	8·86
Chloride of calcium	216·17
Sulphate of calcium	12·27
Silica	3·65
Oxides of iron, alumina, and manganese.	minute quantity
	645·45

("Chemical Examination of a Hot Spring in Huel Clifford, Cornwall, by Professor W. A. Miller, M.D. &c.," Report of the Thirty-fourth Meeting of the British Association &c., held at Bath, September 1864.)

My friend, Mr. W. J. Henwood, has called my attention to the fact that so long ago as 1827 waters rich in chloride of sodium had been found in some of the Cornish mines by Mr. R. W. Fox. This gentleman says*, "In some instances I have detected common salt, particularly in the water from the bottom of the United Mines, the Consolidated Mines†, Huel Unity, and Poldice. Out of the 92 grains of residuum from the latter‡, 24 grains proved to be the muriate of soda, 52 grains the muriates of lime and magnesia, and the remainder muriatic acid with iron and sulphate of lime. The water from another part of the same mine contained $5\frac{1}{2}$ grains of common salt. All these mines are in 'killas' or primitive slate, and several miles from the sea. It may be inferred from such facts as these that the sea-water must in some places penetrate into the fissures of the earth."

Previously to discussing whether the water in question is or is not the result of infiltrations from the sea, it will be instructive to compare, by calculation, its composition with that of sea-water so diluted with distilled water as to contain the same amount of fixed constituents as that found in the Huel Seton spring§. Analyses of the waters of the German, Atlantic, and Pacific oceans agree so closely with one another as to justify the conclusion that the waters of the entire ocean have an essentially similar composition; but for the purpose of this comparison, an analysis of the waters of the Irish Sea by Messrs. T. E. Thorpe and E. H. Morton has been selected||. This analysis has been chosen, both from the circumstance of its recent date, and also because, from the geographical position of the locality whence the water was obtained, it may be regarded as fairly representing the waters of the Atlantic Ocean which wash this portion of the Cornish coast.

* Transactions of the Royal Geological Society of Cornwall, vol. iii. (1827) p. 324.

† Lately part of the Clifford Consolidated Mines.

‡ From the evaporation of a pint of water.

§ The error consequent on the assumption of the dilution being made with distilled water is not great. Four deep-mine waters recently analyzed gave an average of ·42 gramme of solid matter per litre, or less than 3 per cent. of the solid contents of the water in question.

|| Ann. Chem. Pharm. clviii. pp. 122–131.

	Water from the Irish Sea so diluted as to contain the same amount of solid matter per litre as the water from Huel Seton. Grammes per litre (calculated).	Mineral water from Huel Seton. Grammes per litre found (mean).
Chlorine	7·9070	9·1695
Bromine	·0260	trace
Sulphuric acid	·9172	·0178
Silica	....	·0275
Alumina	....	·3458
Ferric oxide	·0019	·0029
Manganese	....	trace
Copper	....	minute trace
Lime	·2441	3·4829
Magnesia	·8627	·0715
Alkaline chlorides	11·5393	6·4773
Potassium	·1665	·0834
Cæsium	....	trace
Lithium	....	·0800
Sodium	4·4157	2·2933
Ammonia	trace	trace
Carbonic acid	·0096	·0790
Nitric acid	·0006	trace

If the Huel Seton spring be regarded as only yielding modified and diluted sea-water, it will be found, on comparing the two columns of the foregoing Table, that the percentage of chlorine in the second has been considerably augmented, that the sulphuric acid has almost entirely disappeared, silica and alumina have been taken up, and the amount of calcium has been greatly increased, whilst magnesium has, on the contrary, been abstracted. The amount of alkaline chlorides has thereby been much reduced; but a considerable quantity of lithium has entered into solution.

It would at first appear that the presence of a much larger amount of chlorine in the waters of the saline spring than should be found in dilute sea-water holding an equal quantity of solid constituents in solution affords an argument against the probability of the Huel Seton waters having had such an origin. If, however, neglecting the smaller differences, the chlorides of calcium and aluminium in the spring-water be, on the one hand, replaced by an equal weight of chloride of sodium, and, on the other, the excess of sulphuric acid in the dilute sea-water be replaced by chlorine, making a correction for the resulting slight difference in the total weight, the amount of chlorine in the two columns will be found very nearly the same. This objection is consequently disposed of.

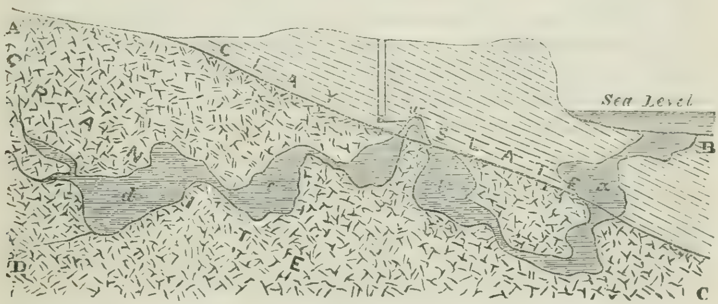
The analyses of the different rocks in the vicinity of the spring have shown that the granite only could have furnished the lithium, of which the waters have taken up a notable amount.

A consideration of the various phenomena connected with the occurrence of this and other apparently similar springs which have at different times been discovered in the district, would seem to lead to the inference that they all have some more or less direct communication with the sea, and that they are either the result of infiltration of sea-water through faults, or are true and independent sources, which, before being tapped below the sea-level, had found their way to the ocean through the faults or channels before alluded to.

It would be impossible to ascertain the precise conditions under which springs of this description have been produced; but the accompanying ideal sketch may perhaps assist in rendering intelligible what would appear, in the present state of our knowledge, a not improbable explanation of their origin.

The plane A B C D (fig. 2), being that of the cross-course, is

Fig. 2.



seen to extend through both granite and clay-slate to the sea. From the close contact of its surfaces, the presence of clay, and from other causes, this fault is supposed not to be uniformly permeable by water, which can only follow the circuitous passage, *a b c d*. In this way it penetrates to depths where reactions take place, which, although not entirely in accordance with the results of daily experience in our laboratories, can, after the investigations of M. Daubrée, M. de Sénarmont, and others, be readily understood.

By the action of sea-water on *silicates of calcium, silicates of sodium* and *chloride of calcium* may be produced. The *sulphate of sodium* of the sea-water will be decomposed by the *chloride of calcium*, with the production of *sulphate of calcium* and *chloride of sodium*. The decomposition of clayey matter by common salt

may produce *chloride of aluminium* and *silicates of sodium*, while the *magnesium* of the chloride of magnesium may be replaced by *calcium*; lastly, a portion of the *potassium* in the sea-water appears to have been replaced by the *lithium* of the granite.

The alteration which has taken place in slates in the vicinity of the cross-course has evidently been attended with the loss of a large percentage of its silica. In fact this appears to have been the chief chemical change effected, since, if we calculate what would be the composition of the clay-slate after the reduction of its percentage of silica to the amount contained in the altered rock, the calculated results will be found to agree very closely with the analysis of the latter. One per cent. of *lime*, however, has been exchanged for *alumina*, and about the same amount of *magnesia* has been substituted for an equivalent quantity of *potassa* and *soda*.

III. *On Negative and Fractional Unitates.* *By* W. H. Walenn*.

THE definition of a unitate, as originally given in the year 1868†, is:—The remainder to any given divisor (usually a digit), determined by a certain theorem, without the knowledge of any multiple of that divisor. The theorem was stated to be:—"If t be the tens' and u the units' digit of a two-figure number, and δ be any integer less than 10, then

$$(10-\delta)t+u$$

has the same remainder to δ as $10t+u$;" and numerous examples of its use were given.

In a paper read to the British Association at Liverpool in 1870‡, the expression $(10-\delta)t+u$ was expanded to

$$(10-\delta)^{n-1}a+(10-\delta)^{n-2}b+(10-\delta)^{n-3}c+\ldots+(10-\delta)^2 s$$
$$+(10-\delta)t+u,$$

n being the number of digits in the given number—so as to adapt it to other than two-figure numbers; and further examples of its use were given.

The examples showed that this theorem is useful, especially when $\delta=9$, in checking calculations and verifying Tables; when other values of δ are used, it is applicable to quickly ascertaining the remainders to divisors.

In his address to the Mathematical Section at the British

* Communicated by the Author.

† Phil. Mag. S. 4. vol. xxxvi. p. 346.

‡ Report of the British Association for the Advancement of Science for 1870, Transactions of the Sections, p. 16.

Association Meeting of 1869 at Exeter, Professor J. J. Sylvester laid much stress upon the employment of inductive philosophy in mathematics. He said that he was aware that many who had not gone deeply into the principles of mathematical science believed that inductive philosophy, or the method of evolving new truths by induction, was reserved for the experimental sciences, and that the methods of investigation in mathematical science might all be classified as deductive. He went on to say that this opinion is not a correct one, and that many valuable results are obtained in mathematical science by induction, or reasoning from particulars to generals, which could not otherwise be obtained so easily. Although making a distinction between mathematical induction and the induction used in natural philosophy, De Morgan, in his article in the 'Penny Cyclopædia' on this subject, states that an instance of mathematical induction occurs in every equation of differences and in every recurring series. Taking the definition of induction as given by Dr. Whateley, namely, "a kind of argument which infers respecting a whole class what has been ascertained respecting one or more individuals of that class," it will be evident to any experimenter in chemical or physical science who is also acquainted with the use of induction in mathematical science, that mathematical induction is of a higher and more perfect kind than the induction used in the physical sciences, especially when it assumes the form of successive induction as De Morgan calls it, and as it is employed in recurring series.

It is this high class of reasoning, which is involved in the construction of series that recur according to a given law, that makes the use of recurring series so valuable in unitation. Nevertheless, in addition to the policy of investigating every general result by the use of a more general application of inductive philosophy, namely by the calculation or examination of particular examples (and so, having by one train of reasoning obtained a general result, proving or illustrating the use of that general result by particular applications), it is especially necessary to study examples of every possible type in unitation. It is moreover necessary to have other means than the principles of recurring series, and of verification by examples, to ascertain the truth of certain results, in consequence of the number of anomalies that present themselves for explanation.

These anomalies occur not only at the ordinary *singular* values that the theory of numbers affords, such as 0 and 1, but the base of the system of unitation—take 9 for instance—together with the submultiples of that base, namely six and three, also furnish singular or anomalous values.

In respect of operations, as might be expected, inverse opera-

tions, such as subtraction and division, when regarded as operations upon unitates, involve mathematical investigation and a rigid comparison of results. These results used for comparison, may either consist of the separate determination of unitates by totally distinct processes, or of individual examples obtained by the application of unitation to checking calculations that are known to be correct.

Applying the principles and modes of thought sketched out above to the examination of the meaning of a negative unitate, the following results are arrived at. It is important to bear in mind certain properties of unitates. 1st. By definition, the unitate of a given number to a given base, say 9, is the remainder obtained by dividing the given number by the base, 9; it must therefore always be a plus or positive quantity, equal to or less than 9. 2nd. Nought (or 0) never occurs in unitation; when there is no remainder the unitate of the number is 9. The three cases into which subtraction of unitates resolves itself are:—(I.) When the unitate of the subtrahend, or number to be subtracted, is less than the unitate of the minuend or quantity to be diminished. In this case the subtraction of the former unitate from the latter leads to a plus or positive unitate, which is the unitate of the remainder. (II.) When the unitate of the subtrahend is equal to that of the minuend. The subtraction leads to 0 as a unitate; this may at once be written 9. (III.) When the unitate of the subtrahend is greater than that of the minuend. Regarded algebraically, this subtraction leads to a negative unitate, and, to obtain its real value, it must be subtracted from 9. Thus when, in unitation, the expression $6-8$ occurs, it may be written -2, but should be at once reduced to a positive quantity by adding 9 to it, thus $9-2=7$, 7 is the unitate required; if it be desired to give the -2 a name, it may be called the co-unitate of 7. For arithmeticians only, it may in practice be the best plan to add 9 to the unitate of the minuend in the last two cases before performing the subtraction of the unitate of the subtrahend therefrom.

The performance of the operation of division upon unitates is derived from the performance of the same operation upon ordinary numbers; but, unlike that operation upon ordinary numbers of which it is the analogue, it always has a remainder, or may be presumed to have one. This is a consequence of no unitate being equal to 0; for instance, in $\frac{25}{5}=5$, the unitate of $\frac{25}{5}=\frac{7}{5}$; the unitate of the quotient 5 may be written $5+\frac{9}{5}$, which is (for the purpose of checking the division of 25 by 5)

equal to $\frac{25+9}{5}$, or $\frac{34}{5}$, or $\frac{7}{5}$. Therefore unitates of all quotients may be obtained by preserving them in the fractional form which they originally have—neither neglecting the remainder, nor reducing any fraction which may thence arise to lower terms, but preserving the exact unitate of the divisor to the very end of the process, either in reducing the unitate of the fraction which expresses the division to be checked, or in reducing the unitate of the quotient (or answer), including the unitate of the remainder. If there be no remainder, the unitated remainder must be a fraction which has 9 for its numerator. Division of unitates, therefore, renders the investigation of fractional unitates necessary.

Since all divisions can be written under the form $a \cdot \frac{1}{b}$, and since $\frac{1}{b}=b^{-1}$, it follows that the question of fractional unitates is identical with that of the unitates of the (-1)th power of numbers, and also with that of the unitates of reciprocals decimally expressed. In the annexed Table of unitates of the powers of numbers, it is observable that the unitates of

Uni. a^{-1}.	Uni. a^0.	Uni. a^1.	Uni. a^2.	Uni. a^3.	Uni. a^4.	Uni. a^5.	Uni. a^6.	Uni. a^7.	Uni. a^8.	Uni. a^9.	Uni. a^{10}.
1	1	1	1	1	1	1	1	1	1	1	1
5	1	2	4	8	7	5	1	2	4	8	7
$\frac{1}{3}$	1	3	9	9	9	9	9	9	9	9	9
7	1	4	7	1	4	7	1	4	7	1	4
2	1	5	7	8	4	2	1	5	7	8	4
$\frac{1}{6}$	1	6	9	9	9	9	9	9	9	9	9
4	1	7	4	1	7	4	1	7	4	1	7
8	1	8	1	8	1	8	1	8	1	8	1
$\frac{1}{9}$	1	9	9	9	9	9	9	9	9	9	9
1	1	1	1	1	1	1	1	1	1	1	1

the same power of different numbers (the vertical columns) repeat themselves after every nine consecutive numbers; also that (with some exceptions presently to be noticed) the unitates of different powers of the same number (the horizontal columns)

repeat themselves after every six consecutive powers. In this way the column for reciprocals, headed $\overset{\text{Uni.}}{a^{-1}}$ is obtained, so far as the numbers 1, 5, 7, 2, 4, 8 are concerned; for these are also the unitates of the corresponding decimals to the reciprocals, the unitate of 7^{-1} or of $\frac{1}{7}$ being obtained from the unitate of the decimal value of $\frac{1}{16}=\cdot0625$. To test the correctness of these unitates, the principle that every number is the reciprocal of its own reciprocal may be applied; accordingly the following equations are true in unitates, bearing in mind that, in the case of unitates, the signs $+$, $-$, $\times$, $\div$, and $=$ have not the same signification as in ordinary arithmetic. The words "the unitate of" are understood to be placed before every numerical value set down.

$$1=\frac{1}{1}=1.$$

$$2=\frac{1}{5}=\frac{9+1}{5}=2.$$

$$3=\frac{1}{\frac{1}{3}}=1\times3=3.$$

$$4=\frac{1}{7}=\frac{(3\times9)+1}{7}=\frac{28}{7}=4.$$

$$5=\frac{1}{2}=\frac{9+1}{2}=\frac{10}{2}=5.$$

$$6=\frac{1}{\frac{1}{6}}=1\times6=6.$$

$$7=\frac{1}{4}=\frac{(3\times9)+1}{4}=\frac{28}{4}=7.$$

$$8=\frac{1}{8}=\frac{(7\times9)+1}{8}=\frac{64}{8}=8.$$

$$9=\frac{1}{\frac{1}{9}}=1\times9=9.$$

Another property of reciprocals that will test these results is, that the product of any number multiplied by its reciprocal is always equal to unity. This is found to be true in carrying out the column headed $\overset{\text{Uni.}}{a^{-1}}$ in the above square Table according to this principle; and it is clear that neither 3, 6, nor 9 multiplied by any other whole number can give a product whose unitate is 1.

The operation of multiplying the unitates by their reciprocals is

$1\times1=1.$	$4\times7=28=10=1.$	$7\times4=28=10=1.$
$2\times5=10=1.$	$5\times2=10=1.$	$8\times8=64=10=1.$
$3\times\frac{1}{3}=\frac{3}{3}=1.$	$6\times\frac{1}{6}=\frac{6}{6}=1.$	$9\times\frac{1}{9}=\frac{9}{9}=1.$

It is to be remarked that the discontinuity of the series in the case of the unitates of the powers of 3, 6, and 9 respectively, which is evident from the second and higher powers not following the same law of recurrence as the first and powers below the first follow, is an indication of departure from a general rule,

which is borne out by the Table of reciprocals calculated by means that are totally independent of series.

From the values that have been thus obtained and verified for the unitates of the reciprocals of 1, 2, 4, 5, 7, 8, it follows that, in any division sum which has these unitates in the divisor, the corresponding unitates of their reciprocals may be written as multipliers to find the unitate of the quotient. For instance, in $\frac{25}{5}=5$, as above given, the following is the readiest process of unitation—the unitate of $\frac{25}{5}=7\times 2=14=5$, and the unitate of $5=5$. This substitution of multiplication for division cannot be made in the case of the unitates of the reciprocals of 3, 6, or 9; therefore in the cases in which 3, 6, or 9 is in the denominator, and in these cases only, it is necessary to preserve the fractional form of the unitate during the whole of the process of unitation.

Thus, considered *in extenso*, subtraction gives birth to co-unitates or negative unitates, and inverse powers, or the operation of division, to fractional unitates, or unitates that are irreducible to the whole number form.

74 Brecknock Road, N.,
June 1873.

IV. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains. By* JAMES D. DANA*.

PART I.

PREPARATORY to a discussion of some questions connected with the earth's contraction, I here present a statement of the views which I have entertained with regard to the prominent results of this agency. They first appeared in 1846 and 1847, in volumes ii., iii., and iv. of the Second Series of the American Journal of Science, and were somewhat extended in 1856 in vol. xxii.† Full credit is given to earlier writers in connexion with the articles referred to. The views are as follows‡:—

* From the American Journal of Science and Arts, vol. v. June 1873. Communicated by the Author, with additional notes.

† Vol. ii. p. 385, iii. pp. 94, 176, 380, iv. p. 88, xxii. pp. 305, 335.

‡ I may add in this place that a sight of Mädler's chart of the moon in 1846, six years after my visit to the crater of Kilauea, in the Wilkes Exploring Expedition, prompted to the first of the articles on the subject (that on the Volcanoes of the Moon, vol. ii. pp. 335, 1846), in which the origin of continents and oceanic basins is considered. The most important

1. The defining of the continental and oceanic areas began with the commencement of the earth's solidification at surface, as proved by the system of progress afterward.

2. The continental areas are the areas of least contraction, and the oceanic basins those of greatest, the former having earliest had a solid crust. After the continental part was thus stiffened and rendered comparatively unyielding, the oceanic part went on cooling, solidifying, and contracting throughout; consequently it became depressed, with the sides of the depression somewhat abrupt. The formation of the oceanic basins and continental areas was thus due to "unequal radial contraction"*.

3. The principal mountain-chains are portions of the earth's crust which have been pushed up, and often crumpled or plicated, by the lateral pressure resulting from the earth's contraction.

4. (*a*) Owing to the lateral pressure† from contraction over

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of Prevost's papers on the origin of mountains had been published six years before, but I knew nothing of his views until after my paper was ready for publication, as I remark in a paragraph near its close.

* The principle thus expressed by Professor LeConte in volume iv. of the same journal (1872) does not differ essentially from my old view, except that it is connected with the idea of a solid globe. Professor LeConte, on p. 466 of his article, attributes to me the opinion that the "sinking of sea-bottoms, determined by interior contraction, is the [source of the] force by which continents are elevated." But I have never referred the origin of continents to such a cause, or to any other than that stated above.

Moreover the elevation of mountains on the borders of continents I have attributed, not to "sinking sea-bottoms" merely, but to lateral pressure produced by contraction over continental as well as oceanic areas, that on the oceanic being made much the greatest, as stated beyond. My language is frequently ambiguous on this last point, because I speak of the oceanic as the "subsiding" areas. But the term is used relatively. In volume iii. on p. 179 (1847) I observe that mountain elevations occur "near the limit between the great contracting and the non-contracting (comparatively non-contracting) areas;" and in various places I describe the contraction as general. In my 'Manual of Geology,' on page 732, I remark that the elevating "force acted most strongly from the oceanic direction," which was the idea throughout. I do not deny, however, that I have supposed too large a part of the lateral force to have come from the special contraction and consequent subsidence of the oceanic part of the globe.

Professor N. S. Shaler, in 1866 (Proc. Boston N. H. Soc. vol. x. p. 237, vol. xi. p. 8, and Geol. Mag. vol. v. p. 511), presented, *as original*, the idea that "mountain-chains are only folds of the outer portion of the crust caused by the contraction of the lower regions of the outer shell," and that "the subsidence of ocean-floors would, by producing fractions and dislocations along shore-lines, tend to originate mountain-chains along sea-borders and approximately parallel to them," which is essentially the view that LeConte attributes to me. These ideas are coupled with others respecting limitations of the action of contraction due to denudation and deposition, in which I have no share.

† In my papers in 1847 I used the terms lateral pressure, lateral force,

both the continental and oceanic areas, and to the fact that the latter are the regions of greatest contraction and subsidence, and that their sides pushed, like the ends of an arch, against the borders of the continents, therefore, along these borders, within 300 to 1000 miles of the coast, a continent experienced its profoundest oscillations of level, had accumulated its thickest deposits of rocks, underwent the most numerous uplifts, fractures, and plications, had raised its highest and longest mountain-chains, and became the scene of the most extensive metamorphic operations, and the most abundant outflows of liquid rock.

And (*b*) since the most numerous and closest plications, the greatest ranges of volcanoes, the largest regions of igneous eruption and metamorphic action exist on the *oceanic slope* of the border mountain-chains instead of the continental, therefore the lateral pressure acted most effectively in a direction *from* the ocean.

(*c*) Since these border features are vastly grander along that border of a continent which faces the largest ocean, therefore the lateral pressure against the sides of a continent was most effective on the border of the largest oceanic basin, and for the two (the Pacific and Atlantic) was approximately proportioned to the extent of the basins,—this being due to the fact that the oceanic were the subsiding areas (that is, those which contracted most), and that the larger area became the most depressed.

5. The oscillations of level that have taken place over the interior of North America, through the geological ages, have in some degree conformed in direction of axis to those of the border regions, all being parts fundamentally of two systems of movements—one dominantly in a direction northwestward or from the Atlantic, the other northeastward or from the Pacific.

6. Owing to the approximate uniformity of direction in the lateral thrust under these two systems through the successive ages (a consequence of the isolated position of the continent between two oceanic basins, transverse to one another in axial direction), mountains *of different ages* on the same border, or part of a border, have approximately the *same trend,* and those *of the same age* on the opposite border (Pacific and Atlantic) have in general *a different and nearly transverse trend.* Hence "one dial plate for the mountains of the world, such as Elie de Beaumont deduced mainly from European geology, will not

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tension, horizontal force, force acting tangentially, as synonyms. "Lateral pressure" was the term oftenest employed, and it was explained by reference to a Prince Rupert's drop. (See Amer. Journ. Sci. 2nd ser. vol. iii. p. 96 &c.) The action appealed to was not in any way different from the "tangential thrust" of Mallet.

mark time for America" (Amer. Journ. Sci., 2nd ser. vol. iii. p. 398, 1847; vol. xxii. p. 346, 1856).

7. The features of the North-American continent were to a great extent defined in præ-Silurian time, the course of the Azoic, from the Great Lakes to Labrador, being that of the Appalachians, and various ridges in the Rocky Mountains foreshadowings of this great chain, and so on in many lines over the continental surface; and thus its adult characteristics were as plainly manifested in its beginnings as are those of a vertebrate in a half-developed embryo.

8. Metamorphism of regions of strata has taken place only during periods of disturbance, or when plication and faults were in progress—all metamorphic regions being regions of disturbed and generally of plicated rocks.

The heat required for alteration came up from the earth's liquid interior. (This part of the view requires modification, while the other part, I believe, remains good.)

9. The volcanoes of the continental areas are mostly confined to the sea-borders, or the oceanic slope of the border mountain-chains—not because of the vicinity of salt water, but because these were the regions of greatest disturbance and fractures through lateral pressure. Volcanoes are indexes of danger, never "safety-valves."

10. Earthquakes were a result of sudden fracturings and dislocations proceeding from lateral pressure. In vol. iii. p. 181 (1847) occurs the remark:—"We see that the lateral pressure exerted would be likely to dislocate;" and in the next line, "such fissurings, whether internal or external, would cause shakings of the earth (*earthquakes*) of great violence, and in all periods of the earth's history, and it might be over a hemisphere at once."

Another important subject (that of the systems in the trends of feature-lines over the globe) is discussed in the articles referred to; but I pass it by for the present.

I propose to bring the above principles under consideration with reference to making such changes as may now be necessary.

I take up, first, the question as to whether oscillations of level (that is, subsidences and elevations) have been made by the lateral pressure resulting from contraction, as is assumed in my writings on the subject and those of most other authors; and how was the lateral thrust from the direction of the oceanic areas made to differ in its results from that from the opposite direction? After which I shall pass to the subjects of metamorphism, igneous eruptions, volcanoes, the earth's interior, and the origin of oceanic basins.

1. *Have subsidences been produced by lateral pressure?*

The theory of Professor James Hall, that the great subsidences of the globe have been made by the gravity of accumulating sediments, has been shown elsewhere* to be wholly at variance with physical law.

Another theory is presented by Professor LeConte in his recent paper in the last volume of the American Journal of Science, to which the reader is referred. Admitting, with Professor Hall, that the mean thickness of the accumulations in the Appalachian region of Pennsylvania is 40,000 feet, and therefore that this is the measure of the gradual subsidence that attended their deposition, he shows that the temperature in the bottom deposits would have been, supposing the usual rate of increase downward (1° F. for 58 feet of descent) 800° F., and at 10,000 feet 230° F.; and he argues that hence there would have resulted below, first, "lithification and therefore increasing density, and therefore *contraction and subsidence pari passu* with the deposit;" next, or at a greater depth, "aqueo-igneous softening" or "melting," the temperature of 800° F. being "certainly sufficient to produce this result as well as metamorphism, and during this process the subsidence would probably continue;" and, in addition, the underlying strata on which the sediments were deposited would have participated in the "aqueo-igneous fusion" and thus have added to the result†.

* Amer. Journ. Sci. 2nd ser. vol. xlii. p. 210 (1866); 3rd ser. vol. v. p. 347 (1873); LeConte, ib. 3rd ser. vol. iv. p. 461 (1872).

† The principal points in Professor Hall's theory of mountains, published in 1859 (Amer. Journ. Sci. 3rd ser. vol. v. p. 347), are:—

1. Coast regions the courses of marine currents, and hence of deposited sediments.

2. The accumulation of sediments by their gravity gradually sink the crust, and thus a great thickness is attained; the rocks become solidified and sometimes crystallized below.

3. The continents afterward somehow raised—not the mountain-regions separately.

4. Shaping of the mountains out of other sediments by denudation.

5. Metamorphism due to "motion," "fermentation," and a little heat; the heat coming up from below (the isogeothermal planes rising) in consequence of the increasing accumulations at surface.

In Professor LeConte's theory (Amer. Journ. Sci. 3rd ser. vol. iv. pp. 345, 460 (1872):—

1. The same as in Professor Hall's.

2. As explained in the text above.

3. After an aqueo-igneous softening of the beds below, the lateral thrust from the earth's contraction pressed together the region of sedimentary accumulation, plicating and crushing the beds.

4. The elevation of mountains due solely to crushing and plication.

5. Metamorphism consequent on the heating derived by the rise of the isogeothermal planes.

No other cause of the gradual subsidence than that here cited is appealed to.

Now *the whole of this contraction took place,* if any occurred, *in the underlying Archæan rocks* (Azoic, or Laurentian and Huronian); for in obtaining by measurement this thickness (40,000 feet) the *contracted* rocks were measured.

The 40,000 feet of subsidence required was therefore wholly independent of contraction in the stratified sediments. But these underlying Archæan rocks were probably crystallized before the Palæozoic era began; for in New York and New Jersey they are in this condition, and they underlie the Silurian rocks unconformably; and the New-Jersey Archæan or Highland region is but a northern part of that of Pennsylvania and Virginia. They would consequently have expanded with the heat instead of contracting. Even if not crystallized, they would have been well compacted under the enormous weight of 40,000 feet of strata; and no experiments on rocks that I have met with authorize the assumption that the ordinary law of expansion from heat would have been set aside.

For further argument on this point I refer to the subsidence in the Connecticut valley during the era of the Connecticut river sandstone (supposed to be Triassico-Jurassic). The thickness of rock produced in the era was probably about 4000 feet; and this is the extent, therefore, of the registered subsidence. The sandstone strata, as is apparent in many places, rest on the upturned metamorphic rocks (gneiss, mica-schist, &c.) of Palæozoic or earlier age. As shown in the preceding paragraph, the contraction, under Professor LeConte's principle, must have been confined to the underlying rocks; and since these are crystalline metamorphic schists, and the depth of sandstone was not sufficient to raise much the temperature within them (the rocks are in general little compacted and often feebly solidified), the heat ascending from below as accumulation went on above would have produced expansion instead of contraction.

Without further reference to facts, it is, I think, clear that the subsidence required could not be obtained by the method appealed to by Professor LeConte. Whatever cause, in either of the above cases, occasioned the subsidence, it must have been one that could do its work in spite of opposition on the part of the heat in the rocks themselves or those below.

Another cause of local subsidence is local cooling beneath, accompanying the increasing accumulation of sediments. But this idea is too obviously absurd to require remark.

In the present state of science, then, no adequate cause of subsidence has been suggested apart from the old one of lateral pressure in the contracting material of the globe.

2. *Have elevations been produced directly by lateral pressure?*

The theory of Professor Hall denies that mountains are a result of *local* elevations, or of any elevation apart from a general continental. This hypothesis I have elsewhere discussed*.

Professor LeConte makes the elevation of mountains real; but after explaining that the crushing effects of lateral thrust would necessarily cause a lengthening upward of the compressed strata (as in the compression of slate rocks attending the production of slaty cleavage), and thereby produce a large amount of actual elevation, arrives at the view that there is no permanent elevation beyond what results from crushing. With crushing, in this action, plication is associated; but it should have a larger place than his words seem to give it (in all plication the rocks over a region being pressed into a narrower space, which could be done only by adding to the height), as it has performed tenfold more work of this kind than crushing.

But are plication and crushing the only methods of producing, under lateral pressure, the actual elevations of mountain-regions? Is there not real elevation besides?

In the later part of the Posttertiary or Quaternary era the region about Montreal was raised nearly 500 feet, as shown by the existence of sea-beaches at that height; and similar evidence proves that the region about Lake Champlain was raised at the same time at least 300 feet, and the coast of Maine 150 to 200 feet. Hence the region raised was large. No crushing or plication of the upper rocks occurred; and none in the under rocks could well have taken place without exhibitions at the surface; and this cause, therefore, cannot account for the elevation. The elevated sea-border deposits of the region are in general horizontal. This example is to the point as much as if a mountain had been made by the elevation.

But we have another example on a mountain-scale, and one of many. Fossiliferous beds over the higher regions of the Rocky Mountains are unquestioned evidence that a large part of this chain has been raised 8000 to 10,000 feet above the ocean-level since the Cretaceous era†. The Cretaceous rocks, to which these fossiliferous beds belong, were upturned in the course of the slowly progressing elevation; and so also were part of the Tertiary beds; for the elevation went forward through the

* Amer. Journ. Sci. 2nd ser. vol. xlii. pp. 205, 252 (1866), and 3rd ser. vol. v. p. 347 (1873).

† The height of the Cretaceous (stratum No. 2 of the Upper Missouri Cretaceous) at Aspen, in Wyoming, is full 8000 feet above tide-level (Meek). Beds occur also in South Park, Colorado, the height of which is 8500 feet; and, according to Hayden, in the region of the Wind-River Mountains the beds have a height of 10,000 to 11,000 feet above the sea.

larger part, or all, of the Tertiary era. But the local crushing or plication of these beds cannot account for the elevation; and no other crushing among the surface-rocks of the mountains can be referred to this era. There may have been a crushing and crumpling of the nether rocks of the mountain. But it must also be admitted that there might have been, under tangential pressure, a bending of the strata without crushing, especially if there is beneath the earth's rind along the continental borders a region or layer of "aqueo-igneous fusion," such as Professor LeConte recognizes.

In the course of the geological history of the North-American continent, there were many oscillations of level in the land. Portions that were raised above the sea-level in one era, in another subsided again and sank beneath it; and Professor LeConte, in the course of his discussion, admits the existence of an elevated region along the Atlantic border which afterward disappeared. Had the elevation in the case of such oscillations been dependent on plication and crushing beneath, so complete a disappearance afterward would have been very improbable.

Such facts as the above appear to prove that elevatory movements have often been, like those of subsidence, among the direct results of lateral pressure. The facts are so well known and the demonstration so generally accepted as complete, that I have suspected that there is here an unintentional omission or oversight in Professor LeConte's paper.

3. *Kinds and Structure of Mountains.*

While mountains and mountain-chains all over the world, and low lands also, have undergone uplifts in the course of their long history that are not explained on the idea that all mountain-elevating is simply what may come from plication or crushing, the *component parts* of mountain-chains, or those simple mountains or mountain-ranges that are *the product of one process of making,* may have received, *at the time of their original making,* no elevation beyond that resulting from plication.

This leads us to a grand distinction in orography hitherto neglected, which is fundamental and of the highest interest in dynamical geology—a distinction between:—

1. A simple or *individual* mountain-mass or range, which is the result of *one process of making,* like an individual in any process of evolution, and which may be distinguished as a *monogenetic* range, being *one in genesis*; and

2. A composite or *polygenetic* range or chain made up of two or more monogenetic ranges combined.

The Appalachian chain (the mountain-region along the Atlantic border of North America) is a polygenetic chain; it

consists, like the Rocky and other mountain-chains, of several monogenetic ranges, the more important of which are:—(1) The Highland range (including the Blue Ridge or parts of it, and the Adirondacks also, if these belong to the same process of making) præ-Silurian in formation. (2) The Green-Mountain range, in western New England and eastern New York, completed essentially after the Lower Silurian era or during its closing period. (3) The Alleghany range, extending from southern New York southwestward to Alabama, and completed immediately after the Carboniferous age.

The making of the Alleghany range was carried forward at first through a long-continued subsidence—a *geosynclinal** (not a *true* synclinal, since the rocks of the bending crust may have had in them many true or simple synclinals as well as anticlinals), and a consequent accumulation of sediments which occupied the whole of Palæozoic time; and it was completed finally in great breakings, faultings, and foldings or plications of the strata along with other results of disturbance. The folds are in several parallel lines, and rise in succession along the chain, one and another dying out after a course each of 10 to 150 miles; and some of them, if the position of the parts which remain after long denudation be taken as evidence, must have had, it has been stated, an altitude of many thousand feet; and there were also faultings of 8000 to 10,000 feet, or, according to Lesley, of 20,000 feet†. This is one example of a *monogenetic* range.

The Green Mountains are another example, in which the history was of the same kind:—first, a slow subsidence or geosynclinal, carried forward in this case during the Lower Silurian era or the larger part of it, and, accompanying it, the deposition of sediments to a thickness equal to the depth of the subsidence; finally, as a result of the subsidence and as the climax in the effects of the pressure producing it, an epoch of plication, crushing, &c. between the sides of the trough.

In the Alleghany range the effects of heat were mostly confined to solidification, the reddening of such sandstones and shaly sandstones as contained a little iron in some form‡, the

* From the Greek γῆ *earth*, and *synclinal*, it being a bend in the earth's crust.

† See an admirable paper on these mountains by Professors W. B. and H. D. Rogers in the Trans. Assoc. Amer. Geol. and Nat. 1840-42. J. P. Lesley gives other facts in his 'Manual of Coal and its Topography,' and in many memoirs in the 'Proceedings of the American Philosophical Society.' A brief account is contained in the author's 'Manual of Geology.'

‡ Oxide of iron produced by a wet process at a temperature even as low as 212° F. is the red oxide, $Fe^2 O^3$, or at least has a red powder. (Am. Journ. Sci. 2nd ser. vol. xliv. p. 292.)

coking of the mineral coal, and probably, on the western outskirts where the movements were small, the distillation of mineral oil, through the heating of shales or limestones containing carbohydrogen material, and its condensation in cavities among overlying strata, with also some metamorphism to the eastward; while in the making of the Green Mountains there was metamorphism over the eastern, middle, and southern portions, and imperfect metamorphism over most of the western side to almost none in some western parts.

Another example is offered by the Triassico-Jurassic region of the Connecticut valley. The process included the same stages in kind as in the preceding cases. It began in a geosynclinal of probably 4000 feet, this much being registered by the thickness of the deposits; but it *stopped short of metamorphism*, the sandstones being only reddened and partially solidified—and *short of plication or crushing*, the strata being only tilted in a monoclinal manner 15° to 25°; it ended in numerous great longitudinal fractures as a final catastrophe from the subsidence, out of which issued the trap (Dolerite) that now makes Mount Holyoke, Mount Tom, and many other ridges along a range of 100 miles*.

These examples exhibit the characteristics of a large class of mountain-masses or ranges. A geosynclinal accompanied by sedimentary depositions, and ending in a catastrophe of plications and solidification, are the essential steps, while metamorphism and igneous ejections are incidental results. The process is one that produces final stability in the mass and its annexation generally to the more stable part of the continent, though not stable against future oscillations of level *of wider range*, nor against denudation.

It is apparent that in such a process of formation elevation by direct uplift of the underlying crust has no necessary place. The attending plications may make elevations on a vast scale; and so also may the shoves upward along the lines of fracture; and crushing may sometimes add to the effect; but elevation from an upward movement of the downward-bent crust is only an incidental concomitant, if it occurs at all.

We perceive thus where the truth lies in Professor LeConte's important principle. It should have in view alone *monogenetic* mountains, and these only *at the time of their making*. It will then read (plication and shovings along fractures being made more prominent than crushing) :—

Plication, shoving along fractures, and crushing are the true

* This history is precisely that which I have given in my 'Manual of Geology,' though without recognizing the parallelism in stages with the history of the Alleghanies.

sources of the elevation that takes place *during the making* of geosynclinal monogenetic mountains.

And the statement of Professor Hall may be made right if we recognize the same distinction, and also reverse the order and causal relation of the two events, accumulation and subsidence; and so make it read:—

Regions of monogenetic mountains were, previously and preparatory to the making of the mountains, areas each of a slowly progressing geosynclinal, and *consequently* of thick accumulations of sediments.

The prominence and importance in orography of the mountain individualities described above as originating through a geosynclinal make it desirable that they should have a distinctive name; and I therefore propose to call a mountain-range of this kind a *synclinorium,* from *synclinal* and the Greek *ὄρος, mountain.*

This brings us to another important distinction in orographic geology, that of a second kind of monogenetic mountain. The *synclinoria* were *made through a progressing geosynclinal.* Those of the second kind, here referred to, were *produced by a progressing geanticlinal.* They are simply the upward bendings in the oscillations of the earth's crust, the geanticlinal waves, and hardly require a special name. Yet, if one is desired, the term *anticlinorium,* the correlate of *synclinorium,* would be appropriate. Many of them have disappeared in the course of the oscillations; and yet some may have been for a time, perhaps millions of years, respectable mountains. The "Cincinnati uplift," extending southwestward from southern Ohio (about Cincinnati) into Tennessee, and referred by Newberry and others to the close of the Lower Silurian, was made at the same time, or nearly, with the Green Mountains; but while the latter range is a synclinorium, the former is a geanticlinal or an anticlinorium, and it is one of the few (probably few) permanent monogenetic elevations of this kind over the earth's surface. There may possibly have been crumpling or crushing in the deep-seated rocks below, which determined its permanence. As far as the Palæozoic rocks constituting it go, it is a *simple* synclinal; but it is really a synclinal of the earth's crust, and hence wholly distinct from ordinary synclinals, or those subordinate among the plications in a synclinorium, like the synclinals of the Alleghanies.

The geosynclinal ranges or synclinoria have experienced in almost all cases since their completion true elevation through great geanticlinal movements, but movements that embraced a wider range of crust than that concerned in the preceding geosynclinal movements, indeed a range of crust that comes strictly

under the designation of a polygenetic mass. Thus the Connecticut-valley sandstone beds, which must have been but little raised by the slight upturning they underwent at the epoch of their disturbance (since there was then neither plication nor crushing), are now 700 feet higher above the sea-level in Massachusetts than near New Haven, Connecticut; and this is owing, not to denudation, but to a subsequent elevation in which much of New England participated—a true geanticlinal uplift. So it has been the world over. The great uplift of the Rocky-Mountain region of more than 8000 feet, which began after the Cretaceous, had nothing to do, as I have said, with crushing or plication, although there was disturbance of the beds in certain local Cretaceous and Tertiary areas; it appears to have been a true geanticlinal elevation of the Rocky-Mountain mass, itself mainly, if not wholly, a combination of synclinoria.

Geosynclinals and geanticlinals of low angle, like those of the present day, graduate insensibly into horizontal surfaces. The later oscillations in the world's history have taken in a vastly wider range of crust than those of early time. We cannot point to any geosynclinal in progress that is probably on the way to become the site of a new synclinorium. This comes from the fact already stated, that the completion of a synclinorium has generally consisted in the solidification as well as plication of the rocks, and the addition of the whole mountain-region to the more stable portion of the earth's crust; and the further fact that this process has been often repeated in past time, until the crust has been so stiffened above, as well as below, that only feeble flexures of vast span are possible, even if the lateral pressure from contraction had not also declined in force.

4. *How was the lateral thrust from the direction of the ocean made to differ in its action or results from that from the opposite direction?*

The fact of a difference in the effects of the lateral thrust from the opposite directions, the oceanic and continental, is beyond question. The evidence may here be repeated.

The greatest of elevations, as well as subsidences, and also of plications and igneous eruptions, have taken place on the continental borders or in their vicinity; they thus show that there is something peculiar along such regions. Again, the border mountains in North America are parallel to the axes of the adjoining oceans, and thereby at right angles, instead of parallel, to one another. Again, the folds in the Appalachians are not symmetrical folds, but, instead, have one slope much steeper than the other, proving inequality in the action of lateral pressure from the continental and oceanic directions. Further, the

larger ranges of uplifts and effects of heat occur on the *oceanic* slope of the principal border mountain-chain instead of the continental slope, favouring the view that this lateral thrust was more effective in the direction from the ocean against the continents than in the opposite. Finally, there is the fact that the disturbances or effects of lateral thrust have been *very much the greatest* on the border of the *largest* oceans.

But has this greater effectiveness of lateral thrust from the direction of the ocean been due to a proportionally greater contraction and subsidence of the oceanic crust than the continental?—the sinking causing the oceanic arch to press against the sides of the basin. I formerly made this the chief means of mountain-lifting; and now, while not giving it so great prominence, I believe it to be a true cause. It is certain that the depressing of the ocean's bed, like the raising of the continental areas, has been in progress through the ages. The great principal rise of the continent and continental mountains took place after the Cretaceous period or during the Tertiary, and some of it even in the Quaternary; and this is almost positive demonstration that the bottoms of the oceans were tending downward contemporaneously. It is not possible in the nature of contraction that it should have been all accomplished in these basins at the beginning of their existence—a point I shall further illustrate when discussing the nature of the earth's interior. Moreover the mobile waters that occupy the oceanic depressions would have given important aid in the cooling of the underlying crust. It is to be noted also that the distance between the axis of the Appalachians in North America and the opposite (African) side of the Atlantic is 4000 miles, and that between the axis of the Rocky Mountains and the opposite (Australian) coast of the Pacific is over 7000 miles; while between the axis of the Appalachians in Virginia and that of the Rocky Mountains in the same latitude the distance is hardly 1500 miles. Hence the contraction was absolutely greatest over the oceanic areas independently of any result from special causes; and if the generated pressure was not expended in uplifts over the oceanic areas themselves, it would be in uplifts on its borders.

In addition to the above advantage which the oceanic areas have had in the making of border oscillations, the lower position of the oceanic crust, and the abruptness with which the sides fall off, give it an opportunity to push beneath the sides of the continents; and this would determine the production of such mountains and just such other effects of pressure on the continental borders as actually exist, even if contraction were equable over the globe—that is, were alike in rate over the oceanic and continental areas. It puts the oscillations over the continents

inevitably under the direction of the adjoining oceanic crust. The angle of slope of the deep-water sides of the oceanic basin is generally above five degrees*.

This conclusion is further sustained by the known universality of oscillations over the oceanic basin. The central Pacific area of coral islands ("registers of subsidence") stretches from the eastern Paumotus to the western Carolines, 90 degrees in longitude, and it indicates that the comparatively recent coral-island subsidence involved a region stretching over more than one fourth the circumference of the globe. The fact teaches that the movements of the globe, which have been in progress through all time in obedience to the irresistible energy generated by contraction, have been world-wide, and so world-developing, even down to the last era of geological history.

The above considerations sustain me in the opinion expressed in 1856 (Amer. Journ. Sci. 2nd ser. vol. xxii. p. 335), that the relation in size between the mountains and the bordering oceans is not merely "formal," as pronounced by my friend Professor LeConte, but has a *dynamical* significance.

In view of the considerations here presented, I believe there is no occasion to reject the fourth proposition, 4 (*a*) on page 42, but only to modify it as follows:—

4. (*a*) Owing to the general contraction of the globe, the greater size of the oceanic than the continental areas, and the greater subsidence from continued contraction over the former than over the latter, and also to the fact that the oceanic crust had the advantage of *leverage*, or, more strictly, of obliquely upward thrust against the borders of the continents because of its lower position, *therefore* along these borders, within 300 to 1000 miles of the coast, &c.

[To be continued.]

* The angle of slope on the sides of the oceanic basin has not yet been properly investigated. The margin of the basin on the Atlantic border is now in about 100 fathoms water (600 feet). According to soundings by the Coast Survey, as I am informed by Mr. A. Lidenkohl of the Coast-Survey Office, through J. E. Hilgard, Esq., Assistant-in-Charge, the slope between 100 and 200 fathoms off Cape Hatteras is 2° 31′, off New York entrance 2° 02′, off George's Shoal 1° 35′. But for the region beyond 200 fathoms the data are not sufficient for any certain conclusion. Mr. Lidenkohl observes:—"If the soundings by Lieutenant Murray off Cape Look-out can be trusted, the slope between the 100- and 2000-fathom line must be over 7 degrees. Berryman's soundings off St. George's Bank indicate a slope of about 3½ degrees. From this it may be inferred that the slope rather increases than decreases beyond the 200-fathom line."

V. *On Thermodiffusion of Gases.*
By W. Feddersen, *of Leipzig**.

THEORETICAL considerations have opportunely been applied by Carl Neumann† to the proposition that if a limited portion of a gas enclosed in a tube of unlimited length (or one that returns into itself) be different in density from the rest of the gas, an artificially produced difference of temperature at the two ends of this portion must occasion a continuous motion throughout the endless cylinder of gas in a determined direction—and, indeed, from the cooler to the warmer end through the limited portion in question, if the gas in this be in the state of condensation.

By experiment I have answered, in a simple manner, to the requirements of the theory. Already in the beginning of this year (1872) I found that experiment confirmed that theory, in that without exception the expected direction of the motion could be recognized. My experiments, however, did not permit the deduction of definite laws—partly because they were in reality only qualitative, in order to prove the fact generally, and partly on account, as it appears, of manifold disturbing influences which were not further ascertained. I therefore thought I must defer their publication until I should have an opportunity of furnishing more exact data. Meanwhile the publication of the results of an investigation by M. Louis Dufour‡ has induced me already, although I have only a few, properly speaking, qualitative results, to publish what I have hitherto discovered, because my observations are the converse of Dufour's and in harmony with them.

My experiments were performed in this manner :—A substance in the form of powder was stuffed tight into a glass tube so as to form therein an immovable plug. This tube was fixed in a horizontal position; and the two ends which projected on each side of the plug were each connected air-tight, by means of caoutchouc, with another horizontal glass tube, which was stopped by a drop of liquid at any place in its interior. In this manner, every displacement of the air cylinder contained in the middle portion of the tubes must displace the two drops of liquid at its extremities in the same direction. One end of the plug was now exposed to a constant source of heat, the other being left cold or artificially cooled. Then, without exception,

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlviii. pp. 302–311.

† *Berichte der Königl. Sächsischen Gesellschaft der Wissenschaften*, Feb. 15, 1872.

‡ *Archives de Genève*, Sept. 1872, pp. 10, 11.

there appeared a slow displacement of the air column in a direction through the plug, from the cold to the heated end, sometimes quicker, sometimes slower, sometimes stronger on one side and sometimes on the other.

Spongy platinum was the first substance with which I experimented, in the expectation that, in consequence of its great absorptive capacity, especially for oxygen, with it rather than any other substance an action would be perceptible. Into a tube of about $3\frac{1}{2}$ millims. diameter, a length of about 60 millims. of spongy platinum was pressed, which had been heated to redness two days previously. When I inserted a drop of mercury in the accessory tube, applied preliminarily to the heated side only, there was indeed a displacement backwards of the drop, but to a considerable degree with strong heating only. In presence of the minuteness of the pressure-force set free, the drop of mercury was evidently too heavy. I substituted for it sulphuric hydrate; and immediately, with the difference of temperatures 10° (that of the room) and 100°, the expected movement began in a striking manner. As an example, out of various experiments I will only mention that, in the eight hours period of observation, the drop once moved 195 millims. away from the heated end. Within this entire interval, however, the speed of the motion varied. In another instance, the platinum being fresh from incandescence, a brisker movement took place. Nevertheless the displacement of the drop was always slow; so that changes in the temperature of the apartment (of course not heated) may have exerted a sensible influence.

I therefore took a shorter tube about $12\frac{1}{2}$ millims. in diameter, inserted therein a plug of recently incandescent spongy platinum of 31 millims. length, and placed at each end a long tube of glass $3\frac{1}{2}$ millims. in diameter, stopped by a drop of sulphuric acid. One end of the plug was exposed to a temperature of about 200°*, while the other end was left to cool freely down to the temperature (8°) of the unwarmed room. On account of the great heat-conducting capacity of the platinum, this end was also not inconsiderably heated. The following were the ascertained displacements in the accessory tubes, all in the direction from the cool, through the plug, to the hotter side:—

* Compare what is said further on, under "*Gypsum.*"

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	
12 0 to 12 10	225	225	tube wanting	millims.
12 10 „ 12 15	94	188	95	190
11 15 „ 12 25	tube wanting	......	241	241
12 25 „ 12 30	105	210	95	190
12 30 „ 12 35	110	220	tube wanting	
Accessory tubes removed, and, after an hour and a half, replaced.				
2 0 to 2 5	73	146	49	98
2 5 „ 2 20	205	137	153	102
2 20 „ 2 30	151	151	112	112
2 30 „ 2 35	76	152	58	116

After the last observation the bent end of the tube on the cold side was immersed in sulphuric acid. The displacements ceased as soon as the acid had risen about 6 millims. in the tube, in which position it remained until, at the end of an hour and a half, I dismounted the apparatus. The capillary rising of the acid in the tube did not amount to one millimetre; so that the difference of pressure produced by the difference of temperature on the two sides of the plug corresponded to a column of sulphuric hydrate of at least 5 millims. height.

Spongy palladium.—In most of my experiments with this I introduced it into an atmosphere of oxygen; but the results obtained were the least regular of any. I had contrived a special arrangement of straight and T-tubes to bring oxygen into the plug-tube as well as into the accessory pieces. In the experiments a movement always showed itself in the expected direction, but with extraordinary variability. The singular irregularities appeared to be provoked chiefly by the variability of the absorption by the palladium with oscillations of temperature; but probably the aqueous vapour, which always formed when the atmospheric air was expelled from the tube by oxygen, was a further cause of the irregularities. Nevertheless the direction of the phenomenon was by all this not reversed.

As an example of the movement the following numbers of one observation may serve: the tubes had from $3\frac{1}{2}$ to 4 millims. internal diameter, and on both sides were stopped by a drop of oil.

Time.	Cold side.	Hot side.
h m h m	millims.	millims.
9 5 to 9 25	130	...
9 25 „ 9 35	155	190
9 45 „ 9 50	45	Aperture touching oil surface raises slight bubbles.

In this example, therefore, between $9^h\ 25^m$ and $9^h\ 35^m$ the oil drop on the hot side removed 190 millims. from the heated part; while that on the cold side moved 155 millims. nearer to the cold end of the palladium.

Gypsum.—In a tube of $12\frac{1}{2}$ millims. internal diameter was a plug of gypsum 70 millims. in length, which, after its pouring-in, had remained some time and become air-dry. The glass tube was encased in sheet copper over one end of the plug; and by a feebly burning spirit-lamp placed beneath, that part was heated to about 200°*, while it was protected from draughts as much as possible by pasteboard walls. The other end of the plug was exposed to the temperature of the room (8°), and manifested, when touched, a very slight warming. On stopping the accessory tubes, $3\frac{1}{2}$ millims. in diameter, with mercury, no movement made its appearance even after fifteen hours. When afterwards sulphuric hydrate was taken for the liquid stop, I obtained the following displacements in the direction from the cold to the hot side:—

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
10 15 to 10 45	tube wanting.	......	60	20·0
10 45 ,, 11 30	,, ,,	......	87	19·3
11 30 ,, 12 15	52	11·5	45	10·0
12 15 ,, 1 15	52	8·7	32	5·3
1 30 ,, 2 0	20	6·7	23	7·7

The aqueous vapour developed from the gypsum, at first in large quantity, and at last only sparingly, did not disturb the direction of the phenomenon.

Charcoal.—Instead of gypsum the tube had, under otherwise like conditions, a plug, 90 millims. in length, of coarsely powdered and recently heated fir charcoal. After the setting-up the drop of sulphuric acid did not at first move at all on the heated side, while on the cold side it moved briskly toward the charcoal.

* The temperature was measured, as the bulb of a thermometer rested on the copper sheath; above, it was somewhat protected from radiation by a loose-lying piece of pasteboard. As is seen, the determination is very closely approximative. The temperature, however, was preserved pretty constant.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
P.M. 2 50 to 3 40	250	50	20	4·0
3 40 „ 4 20	tube wanting.	......	125	31·2
4 25 „ 4 45	200	100	65	32·5
5 0 „ 5 30	200	67·7	30	10·0
5 30 „ 6 10	130	32·5	50	12·5
6 30 „ 7 0	220	73·3	60	20·0
7 0 „ 7 15	73	48·7	23	15·3

During the night the lamp went out, and cooling took place.

A.M. 6 50 to 7 50	285	47·5	tube wanting.	
8 0 „ 9 9	265	44·2	immovable.	
9 0 „ 12 0	tube wanting.	......	415	23·1
12 0 „ 1 0	225	37·5	10	1·7*
1 0 „ 1 30	40	13·3	immovable.	

We see here that in every instance the motion observed in the drop followed the direction through the plug from the cold to the hot end. From the fact that the drop on the cold side always moved quicker than that on the hot side, it must be concluded that, besides the phenomenon of gas-displacement, there was also a special absorption with the charcoal, greater than that which appeared in like manner to take place with the spongy platinum.

Silicic acid.—The arrangement of the experiment was the same as with the two last-named substances. The length of the plug (of silicic acid in powder fresh from incandescence) in the tube of $12\frac{1}{2}$ millims. diameter amounted to 110 millims. After the action of the difference of the temperatures (about 200° and 8°) had lasted continuously some $3\frac{1}{2}$ hours, the following observations were made:—

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
P.M. 5 10 to 6 0	65	13·0	60	12·0
6 10 „ 6 30	90	45·0	75	37·5
6 30 „ 7 10	125	31·3	140	35·0
7 10 „ 7 30	56	28·0	59	29·5
7 30 „ 8 0	64	21·3	75	25·0

* From 12 o'clock the *end* of the tube on the hot side was closed by a drop; the difference of pressure produced by the process was not sufficient to overcome the capillary attraction at that place.

The accessory tubes were taken away at night; yet the difference of temperatures was maintained.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
A.M. 6 40 to 7 10	103	34·3	116	38·7
7 10 „ 7 40	75	25·0	80	26·7
7 40 „ 8 10	74	24·7	82	27·3
8 10 „ 8 40	50	16·7	59	19·7
8 40 „ 9 10	tube wanting.	......	83	27·7
9 10 „ 9 40	„ „	......	76	25·3

Of absorption, such as took place with charcoal and spongy platinum, there is here no trace perceptible.

Calcined magnesia.—The experiment was made with freshly heated magnesia under the same conditions as the last-mentioned substances. In the tube of about $12\frac{1}{2}$ millims. diameter the *length of the plug* was at first 100 millims.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minntes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
5 30 to 5 40	145	145	tube wanting.	
5 40 „ 5 50	140	140	„ „	
5 50 „ 6 0	132	132	95	95
6 0 „ 6 15	214	143	135	90
6 15 „ 6 30	tube wanting.	......	134	89
6 30 „ 6 35	„ „	......	43	86
6 35 „ 6 50	205	137	154	103
6 50 „ 7 0	142	142	tube wanting.	

For the following experiments a part of the magnesia was extracted from the tube, in consequence of which the *length of the plug* was reduced to 40 millims.

Time.	Cold side.		Hot side.	
	Observed.	Calculated for 10 minutes.	Observed.	Calculated for 10 minutes.
h m h m	millims.	millims.	millims.	millims.
7 30 to 7 35	142	284	114	228*
7 35 „ 7 40	140	280	106	212

* In support of the quantity of gas supposed to have passed through the cross section of the plug in the unit of time, let it be remarked that a cylinder of about 13 millims. length in the accessory tubes represents a volume equal to that of a cylinder of 1 millim. length in the diaphragm-tube.

After this, the bent end of the tube on the cold side was immersed in sulphuric hydrate. The sulphuric acid rose quickly to 53 millims. height, while the drop received a displacement corresponding to the dimensions of the closed spaces. The position remained unchanged during the first hour; but after the lapse of the second hour the acid had risen to 62 millims., and after another $1\frac{1}{2}$ hour to about 80 millims., where the bend of the tube commenced; and so any further diminution of pressure in the tube could not be observed. Sustained blowing with the mouth into the open end on the warm side very soon reduced (as was to be expected) the column to zero.

The numbers observed with magnesia, again, point to an absorption of the gas by the substance in the form of powder.

From the preceding experiments with the most heterogeneous substances it appears to follow that it is a universal property of porous bodies, when in the form of diaphragms, to draw gases through them in the direction from the cold to the hot side. We have thus a phenomenon of diffusion which, contrary to ordinary diffusion, occurs even when the same gas under the same pressure is found on both sides of the diaphragm. This is a singular, hitherto unknown phenomenon; and we are therefore justified in giving it the name of

Thermodiffusion.

Whether this kind of diffusion depends also on the nature of the gas, so that with mixed gases a selective diffusion might take place (that is, a mechanical separation of the gases), analogous to that which occurs in the diffusion of liquids, I have not yet been able to discover; it appears at all events probable when Dufour's experiments are taken into consideration. Further, it may be an argument in favour of this view, that I often observed a progressive diminution in the velocity of the motion in the enclosed space, starting from the attachment of the tubes, such as must necessarily occur if, on the cooler side, one of the constituents of atmospheric air diffuses through to the warmer end more quickly than the other; for the closed space on the former side would be sooner exhausted of the more easily diffusing gas than of the other.

Dufour, *l. c.*, states that when gases are caused to diffuse, there is a *rise of temperature* on the side where the more quickly diffusing gas enters the porous diaphragm, and a *fall of temperature* on the opposite side; so that in his experiments diffusion produces a change of temperature; in mine a change of temperature causes diffusion, and the latter in a direction such that the

artificially produced difference of temperatures, if we apply the laws discovered by Dufour, is diminished by the process of diffusion itself. Accordingly we find between thermodiffusion and Dufour's discovery a reciprocity analogous to that between heat and electricity in the ordinary thermo-current and Peltier's phenomenon.

If the forces set free by thermodiffusion appear mostly to be trifling, yet it cannot be forthwith maintained that this phenomenon plays only a quite subordinate part in the economy of nature; for the conditions of its occurrence may at least be widely spread.

Davos, 26th December, 1872.

VI. *Determination of Degrees of Heat in Absolute Measure. By* L. Lorenz, *of Copenhagen**.

ONE of the most important means of modern times, for elucidating the connexion between various forces independently of all physical hypotheses, is the determination of the magnitudes dependent on these forces by the same absolute units. While the system of absolute measures has been carried out in the sciences of magnetism and of electricity, the *degree of heat* has only been determined arbitrarily; and thus, so to speak, the thread has been broken which connects heat with the other physical forces. The object of the present investigation is to establish a definition of the absolute degree of heat in a purely empirical manner, and by introducing it into science to illustrate more clearly the relation in which heat and electricity stand to each other.

The absolute units introduced by Gauss and Weber, which we shall use in the sequel, are the *millimetre* as *unit of length*, the *second* as *unit of time*, and *the milligramme* as *unit of mass*. By means of these units the *electromagnetic* unit of *current-intensity* is defined as being the intensity of that current which, traversing the unit of surface, acts upon a magnetic pole as an infinitely small magnet whose moment is unity. Weber has further defined the unit of the *quantity of electricity* as being that quantity of positive electricity which in the unit of time moves, in the direction of positive electricity, in a current whose intensity is unity, it being presumed that the same quantity of negative electricity is simultaneously transferred in the opposite direction. In what follows we shall, in common with many other authors, define the unit of electricity as being the *sum* of the quantities of positive and negative electricity which, moving in opposite directions through a circuit, produce the unit intensity of current.

* Translated from Poggendorff's *Annalen*, No. 11, 1872.

The absolute *unit of heat* is defined as being the quantity of heat equivalent to the absolute unit of work. If, now, the *degree of heat* be defined as the increase of temperature which produces an absolute unit of heat in being imparted to the unit of mass of water, the degree of heat is arbitrarily determined, since it depends on the physical nature of the substance chosen, namely *water*. If instead of a certain quantity of water we select a certain number of *atoms of an element,* then, in accordance with Dulong and Petit's law, the heating which a given quantity of heat produces in these atoms is independent of the nature of the substance, and it only remains to settle more precisely the number of atoms to be chosen.

The law in question does not hold quite accurately with reference to the *solid* elements; yet the deviations are explained by the fact that heat is not only used for heating, but also for performing internal work. On the contrary, the law certainly holds for all those *gases* in reference to which it can be assumed that none of the heat is consumed in internal work. The loss of heat in external work can be avoided by heating the air under *constant* volume.

According to Regnault, we have for *constant pressure* the specific heats

Nitrogen.	Oxygen.	Hydrogen.
0·24380	0·21751	3·40900

Hence, to heat under constant pressure 14 mgrms. nitrogen, 16 mgrms. oxygen, 1 mgrm. hydrogen, there are required

3·41320	3·48016	3·40900

relative units of heat (1 mgrm. water through 1° C.). These three numbers, of which especially the first and last are very near each other, show, in accordance with Dulong and Petit's law, that the same quantity of heat is required to raise the same volume at the same pressure, and therefore also, as we assume, the same number of atoms of the gases in question, through one degree.

Under *constant volume* the specific heat of these gases is in the ratio 1·40 : 1 less (1·41 on the old, and 1·3945 from Regnault's determinations of the velocity of sound in air); if of the above three numbers we take the mean value of the two which most closely agree with each other (for nitrogen and hydrogen)—that is,

3·4111,

we obtain 2·436 thermal units (1 mgrm. of water through 1° C.) as the quantity of heat required for heating under constant volume as many atoms of a permanent gas as are contained in 1 mgrm. of hydrogen.

The relative unit of heat used here may be readily expressed in absolute units of work; defined in this measure we will express it by A. The unit of heat is equivalent to a work of 433 milligrammetres; and as the weight of a milligramme is 9806 absolute units (that is, the acceleration of gravity expressed in millimetres), we have

$$A = 425 \times 10^7 \text{ absolute units.}$$

But to heat 1 mgrm. hydrogen at constant volume through 1° C.,

$$2{\cdot}436A = 1035 \times 10^7 \text{ absolute units are necessary.}$$

Just as a definite quantity of heat is necessary to raise the same number of atoms of various elements through one degree, so, according to Faraday's *electrolytical law*, equal quantities of electricity are required to separate equivalent quantities from an electrolyte. But as equivalent quantities do not always correspond to the same number of atoms, it is necessary to choose a definite type for electrolysis.

As such a type I prefer the electrolysis of bodies constructed on the formula RCl (Br, S)—partly because an equal number of atoms of the element are separated at each electrode, and partly because we have here the greatest number of atoms which can be separated from an electrolyte by the same quantity of electricity. All deviations from the type taken must then be regarded as arising from secondary actions of the chemical forces. Thus, while we regard the electrolysis of strong hydrochloric acid as normal, the decomposition of water must be regarded as a deviation, which may perhaps be explained by assuming that two atoms of oxygen unite to form a double atom.

In the unit of time an electrical current of unit intensity liberates in a voltameter $\frac{1}{960}$ mgrm. hydrogen*. The same current will liberate from strong hydrochloric acid the same weight of hydrogen and as many atoms of chlorine—that is, will separate at both electrodes as many atoms as are contained in $\frac{1}{480}$ mgrm. hydrogen. To raise the same number of atoms through 1° C. under constant volume, we require, in accordance with what has been said above,

$$\frac{2{\cdot}436}{480} A = 0{\cdot}005075 A = 216 \times 10^5 \text{ absolute units.}$$

We may now define a *degree of heat* in absolute measure as that *increase of temperature* which *the unit of work produces*, in being completely and exclusively changed into heat, *in the same number of atoms of the element which the unit of electricity liberates from an electrolyte under normal circumstances.*

* Conf. Wiedemann, *Die Lehre vom Galvanismus*, second part, p. 917.

From what has been said, this increase of temperature is

$$\frac{1}{216 \times 10^5} \text{ of a Centigrade degree;}$$

and in accordance with the definition given,

$$1 \text{ degree Centigrade} = 0\cdot005075 \text{ A} = 216 \times 10^5 \text{ absolute units.}$$

Besides the connexion between heat and electricity which is expressed by Dulong and Petit's law, and by Faraday's electrolytical law, and which we have used in establishing a definition of the absolute degree of heat, there is another connexion, which found its first expression in Wiedemann and Franz's law, that the conductivity of the various metals for heat and for electricity are in the same ratio. Subsequent investigations have shown, however, that this ratio changes with the temperature, and therefore that the law in its original form cannot be completely valid, but requires a modification.

The influence of heat upon *electrical* conductivity has been investigated by several physicists, as Lenz, Becquerel, Arendtsen, and especially by Matthiessen and von Bose*. These physicists investigated the conductivity of ten different *pure* metals, namely silver, copper, gold, zinc, cadmium, tin, lead, arsenic, antimony, and bismuth. From their investigations the remarkable result was obtained, that the diminution of electrical conductivity for an increase from 0° to 100° C. is the same for all these metals, namely 29·307 per cent. The *resistance* increases, therefore, for the same increase of temperature by 41·46 per cent.—that is, in a somewhat greater ratio than the increase of temperature (36·6 per cent.) if we calculate the temperature from the absolute zero (−273° C.). Matthiessen and Vogt† found subsequently that among the pure metals iron forms an exception, inasmuch as its conductivity may decrease by more than 38 per cent.

But few experiments have been made as to the influence of temperature on conductivity for *heat*. It must, however, be remarked that all the older experiments on thermal conduction agree with the former assumption, that the conductivity is independent of the temperature. Ångström‡ found with reference to two bars of copper, which, however, were probably not quite pure, that between 0° and 100° there was a decrease of conducting-power of from 15 to 21 per cent., and for iron of 28·7 per cent.; while Forbes§ found for bar iron a decrease of 15·7 and 23·3 per cent.

* Phil. Mag. S. 4. vol. xxiv. p. 405.
† Phil. Mag. S. 4. vol. xxvi. p. 542.
‡ *Œfvers. af K. Vetensk. Förhandl.* 1862. Pogg. *Ann.* vol. cxviii. p. 423.
§ Trans. Edinb. Royal Soc. 1862–64.

If we now observe that electrical conductivity for the various pure metals is nearly inversely proportional to the temperature calculated from absolute zero, while their thermal conductivity approaches more to constancy, and further that in the case of iron the deviations for both kinds of conductivity are in the same direction, then we may say that the preceding facts establish, with as great closeness as can be expected, the law that *the conducting-power of a pure metal for heat and for electricity is proportional to the temperature* calculated from absolute zero.

This ratio must obviously more or less vary in several cases. If, for instance, the metal is not homogeneous, or contains admixtures of foreign metals, and, generally speaking, in cases in which, owing to unequal heating in the interior, *thermo-electric currents* may be formed, then the thermal conducting-power will probably be increased, or at all events the relation between the two kinds of conductivity will be altered. This is doubtless the case when heat can be transmitted as *radiant heat* in the interior of bodies. In this kind of transmission we must seek the reason why thermal conductivity for all transparent and translucent media, and generally for all non-metallic bodies, is apparently far greater than that which would correspond to their electrical conductivity. Lastly, if the body is liquid, the ratio must alter, owing to the *mobility of the parts*. If, for instance, a column of liquid is heated from below, this mobility will, of course, increase the observed conductivity. If it is heated from above, currents in the interior cannot be entirely avoided; for every part of the liquid in the same horizontal section cannot have quite the same temperature; the colder parts will then sink, and the warmer ones rise toward the source of heat; and the conducting-power must therefore be *diminished* in consequence of the *motion* of the parts.

We must hold, then, that the law, if it is at all valid, is probably only absolutely so for *pure homogeneous* and *solid metals*. In fact even an unequal heating will make the metals heterogeneous and will give rise to thermo-electrical currents.

From the foregoing observations I shall now attempt to deduce the ratio between the conducting-power of metals for heat and for electricity in absolute units. We shall obtain from this the remarkable result, *that for a pure homogeneous solid metal this ratio is equal to the temperature calculated from the absolute zero expressed in the above-defined absolute units.*

To determine thermal conductivity in absolute measure, we must know how great is the quantity of heat which traverses each unit of surface of a plate of given thickness and with a given difference of temperature at the two sides. The older experiments on this subject have, for very intelligible reasons, led to

results which are not concordant and which are far too low; hence we can only use the experiments of Angström, Forbes, and Neumann, which agree pretty closely with each other, although these observers made their experiments independently of each other and in very different manners. We will for the present use the units chosen by Angström, the centimetre, the minute, the Centigrade scale, and as thermal unit 1 grm. of water 1° C.

Ångström* found for the thermal conductivity of

Copper	58·94 at 0° C.	
Copper	51·63	„
Iron	11·927	„
Mercury	1·061	„

The last determination was made with a mercury column enclosed in a glass tube, which was heated from the top.

Forbes found, in the above-mentioned experiments, for

Iron	12·36 at 0° C.	
Iron	12·42	„
Iron	9·21	„

Neumann† determined the thermal conductivity of five different metal bars, and at the same time made a comparative determination of their relative electrical conductivity. While this was taken at 100 for silver, that of copper was assumed to be equal to 73·3. These results were, with the above units:—

	Thermal conductivity.	Electrical conductivity.	q.
Copper	66·48	73·3	0·907
Brass	18·12	17·9	1·012
Zinc	18·43	21·1	0·873
German silver .	6·566	6·45	1·018
Iron	9·824	10·20	0·963

The ratio between the thermal and the electrical conductivity (which is designated by q) is greatest for brass and German silver. This is not accidental, but probably a consequence of the fact that they are not pure metals. In like manner we obtain, in agreement with what has been said above, a discordant and obviously too low value for mercury; we find for this metal, from Ångström's experiments, $q=0{\cdot}617$, taking as the electrical conductivity of mercury 1·72.

The mean value of the quotients q for copper, zinc, and iron is 0·914 according to Neumann's experiments. Whether, in

* Pogg. *Ann.* vol. cxviii. p. 423, and vol. cxxiii. p. 628.
Ann. de Chim. et de Phys. 1862, vol. lxvi. p. 183.

accordance with other experiments, this number should be made greater or less is difficult to decide; but as the thermal conductivity in Neumann's experiments was not reduced to 0°, q must be taken as a little less for this reason.

Hence, then, we may take

$$q = 0{\cdot}90 \text{ at } 0^\circ \text{ C.}$$

as the result which may be deduced from the present experiments with the greatest degree of probability.

The value of q thus determined is, in Ångström's units, the thermal conductivity of a metal whose electrical conductivity is 1, that of silver being taken as equal to 100. Through each square millimetre of a surface with the thermal conductivity q, whose thickness is 1 millim., there pass in each second

$$q\frac{1}{100}\times 10\times\frac{1}{60}=\frac{q}{600}$$

relative units of heat (1 grm. water 1° C.), with a difference in temperature of 1° C. at the two sides. As the thermal unit used here is equal to 1000 A, and as we have found that 1° C. expressed in absolute units is equal to 0·005075 A, the absolute thermal conductivity corresponding to q, which we will call κ, is defined by

$$\kappa_1=\frac{q}{600}\times\frac{1000 \text{ A}}{0{\cdot}005075 \text{ A}}=328{\cdot}4\,q.$$

From this we see that the factor by which the thermal conductivity is reduced from Ångström's units to absolute measure is independent of A.

With the value we have assumed above for q,

$$\kappa_1=296.$$

If we designate the corresponding absolute electrical conductivity by k_1, we should have, in accordance with the above law,

$$\frac{\kappa_1}{k_1}=\text{T},$$

where T is the temperature calculated from the absolute zero. For the freezing-point of water T is equal to $273\times 1^\circ$ C., and, if a degree Centigrade is expressed in absolute units,

$$\text{T}=1{\cdot}385\text{ A}=589\times 10^7,$$

from which

$$\frac{1}{\kappa}=0{\cdot}00468\text{ A}=1{\cdot}99\times 10^7.$$

If, now, we wish to calculate the absolute resistance of a Siemens's unit (a column of mercury a metre in length and a square millimetre in section at 0° C.), we must know the ratio between

the specific conductivities of silver and mercury. This ratio alters materially with the physical condition of silver; even if, as is most usual, the silver be used in the form of hard-drawn wire, no great accuracy is attainable in the determination of the ratio. In Wiedemann's *Lehre von Galvanismus* (Part I. page 181) we find the numbers 1·739 (E. Becquerel), 1·7 (Lamy), 1·63 (Matthiessen) for the conductivity of mercury, that of silver being taken as 100. Matthiessen subsequently* gave the number 1·65; and Siemens's determinations† gave 1·72 and 1·78. These numbers hold for 0° C.

We may thus take the conductivity of mercury at 0° C. as approximately equal to 1·72; so that, in accordance with the above calculation, the resistance in absolute measure would be

$$\frac{1}{1{\cdot}72k}=0{\cdot}00272\,\mathrm{A}=1{\cdot}16\times10^{7}.$$

Hence Siemens's unit of resistance denoted by S is, in absolute measure,

$$\mathrm{S}=2{\cdot}72\,\mathrm{A}=1{\cdot}16\times10^{10}.$$

We will compare this result with the direct absolute measurements of Siemens's unit of resistance, which have been made partly with the aid of induced currents, and partly by measuring the quantity of heat developed in a conductor by a constant current.

By the first method Weber‡ found

$$\mathrm{S}=1{\cdot}0257\times10^{10},$$

while the Committee of the British Association§ found as a mean

$$\mathrm{S}=0{\cdot}964\times10^{10}.$$

A small correction introduced by Matthiessen, by which both values are reduced 0·3 per cent., is in the present case without significance.

These determinations do not, therefore, materially deviate from the values of S calculated from the thermal conductivity; they are both, however, somewhat lower. The reason for this difference might be sought in the less accurate determination of the thermal conductivity as compared with the electrical conductivity; I think, however, that the reason for this discrepancy is to be sought elsewhere.

The *great* difference, considering the accuracy with which the experiments were made, between Weber's results and those of

* Phil. Mag. S. 4. vol. xxii. p. 195.

† Phil. Mag. S. 4. vol. xxi. p. 25.

‡ *Abh. d. k. Gesellsch. d. Wissensch. Göttingen*, 1862.

§ British Association Report, 1863, Jenkin.

the British Association Committee, a difference which actually amounts to 8 per cent., points to errors which cannot be ascribed to accidental faults of observation, but must rather be ascribed to an imperfect theory. It is here to be observed that the experiments were made with *induction-currents of varying intensity*; yet we may at present undoubtedly state, both from theoretical and experimental investigations, that we know only the elements of the theory of variable induction-currents, and that the results can only be regarded as a first approximation. Hence, notwithstanding the great care with which these measurements have been made, we must not ascribe too great a weight to them.

The determination of the electrical resistance by means of the *disengagement of heat* which a *constant* current produces in a conductor is theoretically far simpler and more certain than the induction method *in the manner in which this has hitherto been applied.* Fortunately we possess a large series of experiments carefully executed and calculated by Quintus Icilius*. This physicist determined the heat which a given intensity of current disengages in a second in various copper and platinum wires, the electrical resistance of which was determined by means of a standard measured by Weber in absolute units. If V denotes the number of relative units of heat (1 mgrm. water 1° C.) which are developed in a second in a Siemens's unit of resistance by the current-intensity S, we obtain by these experiments a determination of the constant α in the equation

$$V=\alpha s^2 \times 1\cdot0257 \times 10^{10},$$

if with Quintus Icilius we use Weber's determination of electrical resistance; while with the former signification of A as the absolute equivalent of work for the relative unit of heat (1 mgrm. water 1° C.), and of s as the absolute value of Siemens's unit of resistance, we have

$$AV=s^2 S.$$

From the two equations we have

$$S=\alpha A \times 1\cdot0257 \times 10^{10}.$$

In the experiments in question three several liquids were used in the calorimeter, namely water, alcohol, and oil of turpentine. The former liquid had the advantage over the two others, that it gave the quantity of heat directly in the units chosen; on the other hand, a small error cannot be avoided, owing to the greater conductivity of water for electricity, by which the disengagement of heat, and therewith also the constant α, must be found rather too small. Owing to its volatility, the experiments with alcohol

* Pogg. *Ann.* vol. ci. p. 69.

showed such small agreement among each other that they must be left out of consideration.

As the average of twenty-eight experiments with *water*, we obtain

$$\alpha = 2{\cdot}543 \times 10^{-10};$$

and of ten experiments with oil of turpentine,

$$\alpha = 2{\cdot}652 \times 10^{-10}.$$

There is no greater difference between these two values of α than might have been expected from the greater conductivity of water; hence the latter number must be taken as that which follows with the greatest probability from Quintus Icilius's experiments. With this value of α we obtain

$$S = 2{\cdot}720A = 1{\cdot}16 \times 10^{10};$$

that is, exactly the same value for Siemens's unit of resistance in absolute measure which we have deduced above from the thermal conductivity of the metals. That it is *exactly* the same value must, of course, be regarded as accidental.

In another way also we obtain a confirmation of the accuracy of the law here propounded, since we shall find that in this law there is the closest agreement between the laws for the propagation of energy in metals, no matter whether this transmission is effected by the motion of heat or of electricity.

By *energy* we understand any magnitude which can be measured by units of *work*. We will consider only the propagation of heat and of electricity so far as it is effected in both cases by *conduction*; so that we may waive any considerations as to the propagation of heat in the interior of a body by radiation and by thermoelectric currents, just as in the case of electricity we neglect the propagation by induction and by thermoelectrical currents.

If by Q we denote the energy present in a body in the unit of volume, the increase $\frac{dQ}{dt}dt$, which Q experiences by *thermal conduction* in the form of heat in the element of time dt, is expressed by

$$\frac{dQ}{dt} = \frac{d}{dx}\kappa\frac{dT}{dx} + \frac{d}{dy}\kappa\frac{dT}{dy} + \frac{d}{dz}\kappa\frac{dT}{dz}, \quad . \quad . \quad (1)$$

where T is the temperature and κ the thermal conductivity, which may in general be regarded as a function of the temperature.

If we substitute in this, in accordance with the law propounded above,

$$\kappa = kT,$$

where k is the electrical conductivity, we obtain

$$2\frac{dQ}{dt}=\frac{d}{dx}k\frac{dT^2}{dx}+\frac{d}{dy}k\frac{dT^2}{dy}+\frac{d}{dz}k\frac{dT^2}{dz}, \quad . \quad . \quad . \quad (2)$$

in which equation all the magnitudes may be considered to be expressed in absolute units.

As the increase of energy here only occurs in the form of heat, it stands in a known relation to the increase of temperature, dependent on the specific gravity and the specific heat; and the equation gives therefore completely the law for the propagation of heat by conduction.

If in any point x, y, z of a body the components of the intensity of the current are u, v, w, and if k is the electrical conductivity, the quantity of heat received by the element of volume $dx\,dy\,dz$ in the element of time dt is, according to Joule's law,

$$\frac{u^2+v^2+w^2}{k}dx\,dy\,dz\,dt.$$

If this element of volume contains at the same time the quantity of electricity $\epsilon\,dx\,dy\,dz$, and if the electrical tension (potential) there is P, the element acquires at the same time the energy

$$P\frac{d\epsilon}{dt}dx\,dy\,dz\,dt$$

in the form of electricity. If, then, as before, Q denotes the energy present in the unit of volume, the increase arising from the motion of electricity, and which occurs both as heat and as electricity, is

$$\frac{dQ}{dt}=P\frac{d\epsilon}{dt}+\frac{u^2+v^2+w^2}{k}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

In like manner, if we disregard the electricity which results from induction, from Ohm's law we have

$$u=-k\frac{dP}{dx}, \quad v=-k\frac{dP}{dy}, \quad w=-k\frac{dP}{dz}, \quad . \quad . \quad (4)$$

in addition to which we have Kirchhoff's equation

$$\frac{d\epsilon}{dt}=-\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right). \quad . \quad . \quad . \quad . \quad . \quad (5)$$

Thus

$$\frac{dQ}{dt}=-P\left(\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}\right)=-\left(u\frac{dP}{dx}+v\frac{dP}{dy}+w\frac{dP}{dz}\right),$$

from which there follows

$$\frac{dQ}{dt}=-\left(\frac{duP}{dx}+\frac{dvP}{dy}+\frac{dwP}{dz}\right). \quad . \quad . \quad . \quad (6)$$

If, owing to the transference of energy, electromotor forces are formed in the interior (thermoelectricity), for no element in which they arise does either Joule's or Ohm's law hold. Yet even in this case the latter equation seems to retain its validity; for it agrees with the observation that a constant current which passes through one section of a conductor from a less to a greater tension produces an absorption of heat which is proportional to the intensity of the current and to the increase of tension.

Yet in this case we disregard any possible thermoelectric currents in the interior of the body, and we obtain from the last equation, by means of equations (4),

$$2\frac{dQ}{dt}=\frac{d}{dx}k\frac{dP^2}{dx}+\frac{d}{dy}k\frac{dP^2}{dy}+\frac{d}{dz}k\frac{dP^2}{dz}. \quad . \quad . \quad (7)$$

Comparing this equation with equation (2), we see that the laws for the propagation of electricity by electrical conduction and by thermal conduction have quite the same form; the positive or negative electrical tension and the absolute temperature calculated from absolute zero will correspond to each other, and, if we choose the absolute measure for the degree Centigrade which has here been proposed, may be measured with the same units. In accordance with these equations a body will receive in each element of its volume the same increase in energy, whether it be unelectrical and have in various places a different absolute temperature T, or whether it be uniformly warmed and have an electrical tension $\pm P$ whose numerical value in each point is equal to T. It is, however, here presupposed that k has in both cases invariably the same value, which is only approximately true. In the next moment the ratio will be materially altered, since the increase of energy in the electrical body occurs in the form of heat, not of electrical tension.

Hence the law for the propagation of electricity cannot be determined by equation (7), which can only serve to define the increase of energy, whereas, as we have seen, the laws for the propagation of heat are defined by equation (2). If electricity continuously moves without change through a body (and this is the only case we can here deal with, as we do not take induced currents into consideration), the quantity of electricity is in each moment the same, and the equation then becomes

$$0=\frac{du}{dx}+\frac{dv}{dy}+\frac{dw}{dz}.$$

This equation, combined with equations (4), gives

$$\frac{d}{dx}k\frac{dP}{dx}+\frac{d}{dy}k\frac{dP}{dy}+\frac{d}{dz}k\frac{dP}{dz}=0. \quad . \quad . \quad . \quad (8)$$

The electrical tension will therefore have to be determined from

this equation in combination with the given limiting conditions.

The motion of electricity is here looked on as permanent; that this may completely be the case, the heat developed (which in this case represents the total increase in energy) must be conducted away. In order now to develop further the analogy between electrical tension and temperature, we will *imagine* the corresponding suppositions applied to the motion of heat in a body; that these suppositions cannot in fact be fulfilled may in this case be disregarded.

We consider a body in which the motion of heat is kept permanent, and in which the entire energy which heat can produce in the form of work in passing from a higher to a lower temperature is conducted away from every point of the body.

If, now, the quantity of heat W at the absolute temperature T is added to a body in every second, and simultaneously and continuously therewith the quantity of heat W_1 at a lower temperature T_1 passes away by conduction, the condition remains unchanged if the entire difference $W-W_1$ is converted into work; and, in accordance with the mechanical theory of heat, the entire amount of work which the quantity of heat W can produce in the change of temperature T to T_1 is obtained if we have

$$\frac{W}{T}=\frac{W_1}{T_1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

If we put for the three coincident surfaces of an infinitely small rectangular parallelepiped

$$\frac{W}{T}=\xi\, dy\, dz+\eta\, dx\, dz+\zeta\, dx\, dy,$$

we have for the other three surfaces

$$\frac{W_1}{T_1}=\left(\xi+\frac{d\xi}{dx}dx\right)dy\, dz+\left(\eta+\frac{d\eta}{dy}\right)dx\, dz+\left(\zeta+\frac{d\zeta}{dz}\right)dx\, dy;$$

and equation (9) will then give

$$\frac{d\xi}{dx}+\frac{d\eta}{dy}+\frac{d\zeta}{dz}=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

In this case $T\xi\, dy\, dz$ is the quantity of heat transmitted through the surface $dy\, dz$ in the unit of time; but this is also defined by $-\kappa\frac{dT}{dx}dy\, dz$ if κ is the thermal conductivity; then

$$\xi=-\frac{\kappa}{T}\times\frac{dT}{dx}=-k\frac{dT}{dx};$$

and in the same manner

$$\eta=-k\frac{dT}{dy}, \quad \zeta=-k\frac{dT}{dz}.$$

In combination with equation (10) these equations give

$$\frac{dk\frac{dT}{dx}}{dx}+\frac{dk\frac{dT}{dy}}{dy}+\frac{dk\frac{dT}{dz}}{dz}=0. \quad . \quad . \quad . \quad (11)$$

We see, therefore, that in the motion of heat which we have here imagined, in which the suppositions are quite analogous to the conditions which actually obtain for the permanent motion of electricity, the temperature would be determinable by the same differential equation as the electrical tension.

By the permanent motion of electricity through a body heat results, which ultimately acquires a permanent motion when it is continuously conducted away in the same manner. The increase of energy which owes its origin *both* to the motion of electricity *and* to that of heat is now *nil* in each element of the body; and from the equations (2) and (7) we shall obtain

$$\frac{d}{dx}\,k\,\frac{d(P^2+T^2)}{dx}+\frac{d}{dy}\,k\,\frac{d(P^2+T^2)}{dy}+\frac{d}{dz}\,k\,\frac{d(P^2+T^2)}{dz}=0. \quad (12)$$

By this equation, in combination with equation (8), both the electrical tension P and the absolute temperature T are to be determined when both the motion of electricity and that of heat are become permanent.

If, for instance, we pass electricity through a body, keeping in a small part σ_0 of its surface a constant electrical tension P_0, and in another part σ_1 of its surface the tension P_1, and if at the same time we maintain both surfaces at the same constant temperature T_0, while the other part of its surface is kept surrounded by perfectly bad conductors of heat and electricity, a permanent motion of electricity and of heat will ultimately result, and the electricity will develop the same heat as is conducted away by the surfaces σ_0 and σ_1.

If we put

$$P^2+T^2+AP+B=\phi, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

in which A and B are two arbitrary constants, we shall obtain by the equations (12) and (8),

$$\frac{d}{dx}\,k\,\frac{d\phi}{dx}+\frac{d}{dy}\,k\,\frac{d\phi}{dy}+\frac{d}{dz}\,k\,\frac{d\phi}{dz}=0. \quad . \quad . \quad (14)$$

Both constants A and B will then be so determined that for both surfaces σ_0 and σ_1 $\phi=0$, by putting

$$P_0^2+T_0^2+AP_0+B=0,$$

$$P_1^2+T_0^2+AP_1+B=0.$$

With the values of A and B which result from these, namely

$$A=-(P_0+P_1) \text{ and } B=P_0P_1-T_0^2, \quad . \quad . \quad . \quad (15)$$

we shall have for all points of the body

$$\phi=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

since here both the differential equation (14) and the limiting conditions in the surfaces σ_0 and σ_1 are satisfied, while the limits for the rest of the surface of the body, where k is *nil*, are satisfied by any given value of ϕ, and therefore as well as by that here taken. From equations (13), (15), and (16) we have

$$T^2-T_0^2=(P_0-P)(P-P_1). \quad . \quad . \quad . \quad . \quad (17)$$

If, then, a constant electrical current be passed for some time through a conductor of any shape which is surrounded by bad conductors, and if the temperature at the two surfaces be kept constant and equal, it will be possible by equation (17) to calculate the temperature at each point of the conductor from the two differences of electrical tension at the given point and the conducting surfaces. The result found will, conversely, serve for an experimental determination of the Centigrade degree in absolute units.

The increase of temperature which depends on the electrical current is $T-T_0$. Now

$$T^2-T_0^2 > (T-T_0)^2;$$

and in accordance with the last equation,

$$(T-T_0)^2<(P_0-P)(P-P_1).$$

As the right-hand side acquires its greatest value for $2P=P_0+P_1$, if P_0 is taken as greater than P_1, we have also

$$T-T_0<\frac{P_0-P_1}{2}. \quad . \quad . \quad . \quad . \quad . \quad (18)$$

From this we see that the greatest increase of temperature which can result in any point of the circuit is always numerically smaller than half the difference of the electrical tensions in the two conductor-surfaces. It would be *equal* to just half this difference if the conductor-surfaces could be cooled to the absolute zero—that is, for $T_0=0$. Thus the differences of electrical tension, and the greatest increase of temperature which can be obtained by them, stand in the closest connexion with each other.

Hence it is not without interest to calculate the difference of electrical tension in the poles of a voltaic element for instance, the electromotive force of the element, in Centigrade degrees. Thus the electromotive force of a Daniell's element is about 12×10^{10} absolute units; half the difference of tension of the elements, or, as it is commonly expressed, the positive tension of the element (the negative taken at the same amount), is therefore 6×10^{10}

absolute units, or (since $1°$ C. is equal to 216×10^5) $2780°$ C. This would thus be the greatest increase of temperature which the element could produce with a constant current in a circuit if the element itself were kept at a constant temperature. This increase of temperature would in fact only take place if the element or its poles could be cooled down to absolute zero; if, on the other hand, we assume for this the temperature of $20°$ C. above the freezing-point of water, we shall obtain from equation (17) $2502°$ C. as the greatest increase of temperature.

In a thermoelectric element of copper-pyrites and copper investigated by Bunsen*, the electromotive force was about equal to one tenth of a Daniell's element; the positive tension therefore was about equal to $278°$ C. when one junction was heated to the melting-point of tin, and the other to about $60°$ C. above the freezing-point of water. In fact the greatest increase of temperature, if it were calculated as above, would be about $111°$ C.

Yet it cannot be concluded from this that, for instance, the last-named element could not produce a spark (and consequently a far greater heating) through the interruption of the circuit; I am convinced, on the contrary, that this is possible. To show with what extraordinary facility the electrical spark can result from a break in the circuit, I will adduce the following experiment. An electrical current, whose intensity in absolute measure was 20, was passed through a copper wire 1 millim. in thickness. This was connected with a sharp knife-edge, with which another part of the wire was scraped with rapid strokes. It was then seen that in complete darkness there was still a bright space between the steel knife-edge and the copper wire when the distance between the two points of the copper wire which were placed by the knife-edge in conducting communication was only 400 millims. This facility with which a spark results from the interruption of the current, however, shows only that *induction* here plays an important part.

VII. *Notices respecting New Books.*

Light Science for Leisure Hours. Second Series.
By Richard A. Proctor, *B.A.* London: Longmans and Co.

Mr. PROCTOR tells us in his titlepage, quoting from Tennyson, "the truths of science [are] waiting to be caught;" and numerous at the present day are the fishers for these truths. In the work before us the author performs the office of urging scientific facts combined with his own and others' opinions into the nets prepared and spread for their reception, these nets being the minds of his readers. In the short notice which we propose to give of this

* Pogg. *Ann.* vol. cxxiii. p. 505.

second series, it will be our object to draw the attention of the reader to the mental process which he ought to perform while perusing the familiar essays presented to him: he must gather of every kind of opinion; indeed he cannot fail to do so; but it is incumbent on him to store up that which is good and to cast the bad away.

These thoughts have been suggested by the controversial spirit which generally pervades the essays. So important is it to present the truths which science seeks in a familiar form to the uninitiated, that, after carefully perusing the volume, we pondered for some interval of time over the title itself, "Light Science" for "Leisure Hours," and were almost inclined to come to the conclusion that it partook of the nature of a misnomer. Turning to the essays entitled "The ever-widening World of Stars" and "Movements in the Star-depths," we could not find in either the characteristic of *lightness*. With so masterly a hand have they been written that every step the author takes to establish the point he aims at (that of the unity of the sidereal universe) is of the most sterling character. To regard either of them as *light* is doing injustice to the sublime science of which they form a part; and we feel certain that if our leisure hours are to be occupied by that which is "light" (we cannot help adding the concluding part of the sentence, "and trifling"), then Mr. Proctor's Essays are not the pabulum for minds that seek for light reading in their leisure hours; for there is not one essay in the volume but requires thought, and deep thought too, ere the reader can gather up the good and cast the bad away. Perhaps Mr. Proctor adopted the title from a desire to catch the nets that they might catch the truths.

In the author's sketch of the life and works of Mrs. Somerville, we find him giving expression to his conviction that, as respects the main purpose of her great work, 'The Mechanism of the Heavens,' Mrs. Somerville failed entirely; and as explanatory of so remarkable a conclusion, one, be it remembered, opposed to the opinion of the late Sir John Herschel, which is quoted by Mr. Proctor, the author alleges that in his opinion success was altogether impossible, and also that "the thorough training, the scholarly discipline which can alone give to the mind the power of advancing beyond the point up to which it had followed the guidance of others, had unfortunately been denied to her." Otherwise, and under happier auspices, our author intimates that Mrs. Somerville might have done original work. Mr. Proctor, we apprehend, has had the advantage of this thorough training, and has experienced this scholarly discipline, and therefore is on the road for executing much original work; but has he ever heard of men or women being beyond the age in which they lived and worked? Have *all* who occupy niches in the great temple of Science had their names enrolled in the records of universities? or have the pioneers in the perpetual siege which has been for ages and is still carried on with the view of acquiring a knowledge of the great forces operating in the Natural World, *always* been men who have first distinguished

themselves by their proficiency in their academical studies? Where are the self-taught men who have even left behind them in the race others who have had the advantages of which Mr. Proctor speaks? We cannot agree with the author even in the slightest manner depreciating the work of so gifted a woman as Mary Somerville; and it might have been that, had she undergone the discipline of which he speaks, she might have found herself hampered, even as when she endeavoured to please Mr. Murray she considered that she departed from clearness and simplicity. We fully agree with her own estimate, that the truths of science are in themselves not so formidable as most persons imagine; and, as regards the popularizing of science, so dear to Mr. Proctor, we much question whether the course adopted and greatly patronized in the present day is one calculated to extend the basis of the pyramid of knowledge. Is it not rather aiming at increasing its height? and do we not find in our ordinary intercourse with mankind that the knowledge is in the books and but seldom in the minds of men?

There are many passages in the Essays to which we should like particularly to direct the attention of the reader, both for their excellence and also for the controversial aspects which they bear; but our space reminds us that to the reader himself we must leave them, simply advising him to use his own judgment, and to read the work not as one which he would take up to while away a leisure hour, but one requiring patient and steady thought, and especially to weigh the author's opinions, which in some cases we think he will find far from being "light." The Essays are calculated to advance science, not so much by the author's endeavour to present it in an attractive form, as by leading the thoughtful reader to test every opinion presented in them.

Selections from the Portfolios of the Editor of the Lunar Map and Catalogue. London: Taylor and Francis.

This Selection, in which it is intended to place on permanent record observations of the physical aspects of lunar objects, contains papers by the Rev. T. W. Webb, Edmund Neison, Henry Pratt, and the Editor, which treat more or less on changes on the moon's surface, a knowledge of which is increasing in interest. These are followed by illustrated notices of recent observations, and comparisons of these with older observations. We hope this attempt to bring observers of the moon into communication with each other will meet with the support it deserves. From the great heat which the moon's surface attains under its long day it is likely that change is continually occurring upon it.

VIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xlv. p. 391.]

March 13, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"Visible Direction: being an Elementary Contribution to the Study of Monocular and Binocular Vision." By James Jago, M.D. Oxon., A.B. Cantab., F.R.S.

It is a well-known fact that when the eye has been displaced in its socket, as, for instance, by the tip of the finger applied to the eyeball through the eyelid, all objects seen by it deviate from their true directions; and the author's mode of proceeding in this paper is to inquire whether visual deviations that may be observed in arbitrary, but methodically devised, displacements of the eyeball in its socket follow any law, and then to consider how far the results thus derived are conformable with other monocular and binocular experiences, and how far they may be available in the explanation of certain phenomena that have been deemed anomalous in physiological optics.

Having pointed out means by which the ball may be easily displaced in any direction, he draws attention to the fact that, when by such means the apparent directions of objects seen by the eye are made to deviate from their true directions through fully 30°, the orbital muscles so fully retain their command over the movements of the eyeball, that that point in the visual field which was painted on the point of direct sight in the centre of the foramen centrale retinæ still continues to be there painted. He shows this to happen whatever be the direction in which the eyeball is displaced in its orbit.

This fact being a fundamental one in the inquiry he has in hand, he puts it to nicer tests still.

He adjusts the two eyes, when equally displaced so as to cause objects to deviate greatly from their true directions, to look awhile at the top of a high object in the open air, and having obtained a strong *spectrum* of this object in the retinæ, he, with the released eyes, looks at an appropriate mark on a grey wall, and finds that the spectrum really has its margin across the point of direct sight; and he tries other experiments in corroboration.

Also by agitating a pin-hole in a card across the eye when looking at such a high object, he brings the retina into view, and sees that the point of direct sight is visibly within the foramen centrale retinæ, as made visible by the shadow of the wall that bounds the foramen. He indicates other means of proving the same fact.

He gathers from a series of experiments that the mastery of the orbital muscles over such movements of the eyeball as are requisite for pointing the optic axis to its objective point, is practically

unimpaired by such shiftings of the eyeball in its socket as have been described.

He then proceeds to show that the regulating duties of the orbital muscles, when the eyeball is displaced in its orbit, are not only fulfilled as to the rotation of the optic axis about a central point, but as to the rotation of the eyeball about this axis. To make experiments to this end, we must have another subjectively visible retinal spot besides the foramen centrale; and this we negatively have in the punctum cæcum, or at the base of the optic nerve.

A diagram is devised by which we may manage that one point of it shall be seen by the direct sight of both eyes, whilst another point is found to fall in the middle of the blind spot of one eye; and the diagram is examined by this eye when this has been pushed from its orbital place upwards, downwards, inwards, and outwards, and in various oblique directions, besides when more or less twisted on its axis; and thus it is demonstrated that what happened with the point of direct sight in the retina happens equally surely for every other retinal point—that under all these displacements the orbital muscles do not forfeit their control of the eyeball, but so regulate its movements that the different points of the field of vision remain constantly painted on the same retinal points.

From these and other methodically continued experiments, he draws the general inference, that if the centre of the foramen centrale retinæ be forced at any instant from its position by any sort of manipulation, and then made to describe a circle round its first position of ease whilst the optic axis has never ceased to remain parallel to its first direction (that is, has generated a cylinder in revolving), the axis of the *seeming* field of vision will have so revolved as to have generated a cone, whose apex is posterior to the retina in the first or undisturbed direction of the optic axis. The like might have been said of any other normal to the retina, the axis of the base of the optic nerve, for instance, were it accessible to light, whilst a twisting retinal movement about a fixed axis twists the *seeming* field of vision.

If the optic axis revolve so as to generate a cone whose apex is in front of the eye, the axis of the *seeming* field may, according to circumstances, generate a cylinder, or a more acute cone enclosing the other.

Conversely, the parallax of the visual field being noted, we can assign the retinal displacements that have produced them.

Should undue contraction of any orbital muscle, or discordant contractions of the orbital muscles, engender visual parallaxes, we may as safely judge from these parallaxes of the retinal displacements that must have been induced, as if they had been due to manipulation of the eyeball.

In these summarized conclusions we have the means of solving highly important problems in physiological optics.

It is found that sensation, or the function of responding to objective light, is exclusively resident in the retinal elements of the bacillar layer, but that the visual functions of the retina extend no

further; for it has been evinced in manifold experiments that when the axes of all the pencils of objective light which concur in imaging a picture upon the retina are normals to its surface, any point in the picture may be *perceived* as lying in successive directions, forming very variable angles with and round about its normal. The retina cannot inform us of the visible direction of any point painted on its surface.

This being so, there is no alternative but to seek for a solution of the mystery in the structure out of which the retina proceeds, for the property in question is plainly inherent in the visual nervous apparatus.

The author recalls that he had long ago pointed out that, though the optic nerve in its orbital course and its fibres in their retinal course are obnoxious to mechanical pressure, no visual sensation can be immediately produced by such pressure on nerve-trunks or branches. Sensation can only be produced by pressure through the sclerotic by affecting the rods and cones of the bacillar layer, and then only when the flexure of the retina crowds together the *internal* (as to the eye) ends of the bacillar elements. He cites his former words:—"When we turn in the dark the eyeballs sharply, or even mildly, a couple of white circular rings, brighter at one margin than the other, enclosing a paler area with a central dark spot, flash forth, the diameter extending an angle of several degrees. . . . . . The phenomenon is plainly the result of flexure of the retina where the nerve runs into it, as the eye is pulled round in its socket until it drags upon the nerve; and it is to be noted that it is *again* where the inner retinal elements are squeezed laterally that the phenomenon is disclosed." The absence of the tough and dense sclerotic where the nerve penetrates it, as well as of the choroid, indicates how readily the nerve must yield to the slightest traction.

In these previously recorded facts the author feels assured that he had, unwittingly, provided himself with a key to the secret of visible direction.

For it has been shown by diversified experiments that whenever there is a parallax in visible direction it is accompanied by a displacement of the base of the optic nerve in the same direction—that is to say, by *traction* upon the nerve-stem, tending to carry its distal extremity that way. The "white circular rings, brighter at one margin than the other," have been instanced as proclaiming that such traction cannot occur without flexure between the nerve-stem and the eye-apple, which displays itself at the junction of the optic disk with the surrounding retinal expansion. In other words, under the concordant action of the orbital muscles, all the movements of the globe are so equably coordinated that the nerve-stem is never subjected to unwonted traction, and consequently always emerges through the ocular tissues to open out into the retina as a normal to their surfaces, in which case no visual parallax appears. But no sooner is there lateral traction than the axis of the emergent nerve-stem, or of the optic disk, deviates from the said normality; and were that disk impressible by objective light, its central point

would deviate in the same direction, and an equal deviation in visible direction would be associated with every other point in the visual field.

Hence we are fairly landed upon the conclusion that visible direction, which has already been tracked backwards to the optic nerve, is a function of its terminal direction, being identical with it at the centre of the optic disk, both in the equable use of the eye and in the unequable.

Finally, it is clear that if the eyeball be twisted round the axis of the optic disk the terminal portion of the nerve will be twisted in the same direction; and thus the opposite twisting of the visible field in certain experiments related are explicable by the same hypothesis—an hypothesis that accounts for all the phenomena of visible direction, whether regular or irregular.

Whenever the inverted retinal image, by means of nervous arrangement, is *re*inverted, an erect image is seemingly projected, if not from, by means of the base of the optic nerve.

The principles here arrived at, when applied to binocular vision, lead to the observation of phenomena that have not been before put on record.

Wheatstone, in his classic paper in the Philosophical Transactions, wherein he announces his discovery of the stereoscope and expounds its theory, only speaks of stereoscopic vision from two perspectives, an appropriate one for each eye, when (no instrument being used) the optic axes meet each other beyond them, or have previously intersected, so that each eye sees the other's perspective; that is, in all experiments by him and other subsequent writers on the subject the optic axes have always been supposed to intersect or to lie in one plane.

But as it has been demonstrated that the axes of visible direction need not be coincident with the optic axes, it ought to follow that we may continue to see bodies in relief from a pair of stereoscopic perspectives, though these are not placed transversely to each other.

Two perspectives of a pyramid are drawn, such as, when placed laterally apart as is usual in stereoscopic slides, and looked at by concourse of the optic axes beyond them, yield a hollow pyramid, and when looked at by a previous decussation of these axes yield a solid pyramid. But these perspectives are placed so that the one which was at the left has the one that was at the right immediately underneath it, with about half an inch of plain paper between them.

Then it comes to pass that by properly displacing the right eyeball upwards, by means of the tip of the finger placed underneath it, we can put the under perspective immediately upon the upper one seen with the other eye, and thus realize the hollow pyramid; or by placing the finger upon the top of the left eye, we can depress the upper perspective to cover the under one, and thus realize the solid pyramid. The first pyramid depends from the plane of the paper, the second stands upon it.

By means of a finger under one eye and another upon the other.

we can obtain either hollow or solid pyramids anywhere between the two perspectives; or by a finger on both eyes, or under both, we can obtain the pyramid and its "converse" below or above both perspective outlines.

In all these cases the optic axes do not intercept each other, but the axes of visible direction (functional of the final directions of the optic nerves) do meet on the paper; that is, the first pair of axes are not, and the second pair are, in such a realization as is herein planned, in one plane.

In all these cases the perspectives fall on similar parts of the two retinæ, as in the modes originally mentioned by Wheatstone.

The author goes on to consider in what way the sensorium refers the sensations it receives notice of from the optic nerves into space, so as to fix the place, size, and form of an object.

The theory of vision in retinal normals being proved to be untenable, it is admitted that there is some such association of the two retinæ as to have fairly suggested the theory of "identical" or "covering" points; but this relation he believes to be subordinate to a law by which the sensorium *projects* or *emits* its perceptions into space, as it were in two imaginary cones of *sight-rays*, which, though not issuing from the ends of the optic nerves, have apices whose positions are functional of the directions of these ends for the instant in question—and that it is by the intersection of the sight-rays in these cones, limited by the law of similar retinal parts, that the places, sizes, and forms of objects are determined. Hence, if we conceive that a pair of stereoscopic perspectives, one being imaged on one retina and one on the other, exist as sight-affections in miniature in the substance of the optic nerves, the size of the resultant solid form will be greater the greater is the distance from the nerves at which the axes of visible direction intersect, or the optic axes when they are coincident respectively with them.

The paper concludes by exemplifying in sundry ways the modes in which the conclusions in it may be applied in investigating seemingly anomalous phenomena in physiological optics.

IX. *Intelligence and Miscellaneous Articles.*

INDUCED CURRENTS AND DERIVED CIRCUITS.

BY JOHN TROWBRIDGE.

THE expression for the intensity of an induced current, deduced by Neumann and Sir William Thomson, is as follows:—$i=\frac{1}{k}\frac{dU}{dt}$, in which k is a coefficient depending upon the resistance of the complete wire in the secondary circuit, and U is a certain "force-function" which depends solely upon the form and position of the wire at any instant, and on the magnetism of the influencing body. The expression, in general language, is as follows:—

"When a current is induced in a closed wire by a magnet in relative motion, the intensity of the current produced is proportional

to the actual rate of variation of the 'force-function' by the differential coefficients of which the mutual action between the magnet and the wire would be represented if the intensity of the current in the wire were unity."

This investigation was undertaken to ascertain if the laws of derived circuits apply to the currents of induction, which are represented by equations of which the above is a type. A reflecting galvanometer of large resistance was included in the secondary circuit, and connected by copper wires of very small resistance with the coil in which the secondary currents were produced: the resistance of these wires was infinitesimal in comparison with the resistance of the galvanometer. The galvanometer was then shunted. The first two columns of the following Table show that, with an inappreciable resistance outside of the galvanometer coils, the shunts made no difference in the deflection of the galvanometer-needle when the shunts were not less than three ohms. Below this the current divided. The resistance of the galvanometer was 5880 ohms; and the last numbers in the second and third columns show that an equal impulse was transmitted through both the shunt and

Exterior Resistances, in ohms.	Shunts, in ohms.	Deflections.	Exterior Resistances, in ohms.	Deflections.
0	3	210	10	210
0	4	210	20	210
0	5	210	30	210
0	6	210	40	210
0	5880	210	100	190

the galvanometer; for no reason can be assigned why it should take one course in preference to the other. Two galvanometers, therefore, of the same resistance, one forming the shunt to the other, will give the same deflection, which is equal to that given by the undivided circuit.

Resistances were then introduced into the circuit exterior to the galvanometer-coils, and a shunt of 588 ohms was used.

The fifth column shows that no effect was produced by the shunt until the exterior resistance was appreciable in comparison with that of the galvanometer.

The following Table exhibits the effect of resistances which were appreciable in comparison with the galvanometer-resistance. The same shunt of 588 ohms was used. The second column is calculated on the assumption that $i = \frac{dU}{dt}$ (where k^1 is a coefficient) is equivalent to $i_1 = \frac{E}{R}$, and that the laws of Kirchhoff hold. The third column is obtained from the experimental data. The fourth and fifth columns are also calculated on the assumption that $i = \frac{E}{R}$

=tangent of the deflection. Columns second and third are expressed in arbitrary scale-divisions.

Exterior Resistances, in ohms.	Calculated value of i.	Experimental value of i.	Ratio of Intensities.	Ratio of Tangents.
1500	1242	1375		
2000	1033	1055	1·06	1·03
2600	954	990	1·06	1·04
3000	767	825	1·06	1·04
3500	673	770	1·11	1·05
4000	613	660	1·05	1·03
4500	558	649	1·05	1·07
5000	514	550	1·04	1·03

It will be seen by comparison that, with large resistances exterior to the galvanometer-resistance and appreciable in connexion with it, the laws of the division of currents practically hold, and as the exterior resistance approaches that of the galvanometer the coincidence with the laws is more marked.

From the above it appears that, under certain conditions, an induced current does not divide according to the laws of divided circuits, but approximates to these laws when there is a resistance exterior to the galvanometer which is appreciable in comparison with that of the galvanometer.—Silliman's *American Journal*, May 1873.

ELECTRICAL FIGURES ON CONDUCTORS. BY H. SCHNEEBELI.

M. Schneebeli has investigated the conditions on which depend the dimensions of Kundt's electrical figures, which result from the adhesion of a fine insulating powder upon a metallic conductor from which a discharge has just issued*. In his experiments, the discharge of a Leyden jar took place between a horizontal metal plate, sprinkled with Lycopodium-powder (for the production of the electrical figures), and an electrode in the form of a knob, cone, or point surmounting it. The author has found, like M. Kundt, that, *cæteris paribus*, the diameter of the figure increases with the distance of the electrode from the plate, but not in a constant ratio; the line which represents the ratios is not straight, but an undulated curve. Also the size of the figure augments with the quantity of electricity which produces it.

When the electrode is composed of a certain number of points, a circular figure is formed beneath each of them. If a small disk of glass is interposed in the path of the discharge, there is produced

* Kundt's figures are produced with great neatness on the positive electrode, but are only obtained with difficulty upon a plate serving as the negative electrode. That physicist found that the diameter of the figures increases with the length of the discharge, and in proportion as the diameter of the opposite electrode to the plate diminishes (*Archives des Sciences*, 1869, vol. xxxv. p. 212).

on the plate a space void of powder and having exactly the shape of the disk.

With electrodes of a conical form with an angle of 60° or of 30°, or in the shape of a tapering needle, M. Schneebeli ascertained that the electrical figure is as much larger as the angle at the summit of the cone is smaller.

Lastly, the diameter of the figure is greater when the discharge is effected in a rarefied gas than at the ordinary pressure.—*Archives des Sciences Physiques et Naturelles*, vol. xlvi. p. 269.

NOTE ON GASEOUS PRESSURE. BY ROBERT MOON, M.A., HONORARY FELLOW OF QUEEN'S COLLEGE, CAMBRIDGE.

As Mr. Strutt appears indisposed to continue the controversy between us*, it may perhaps be permitted me to recapitulate its results.

Mr. Strutt impugned my views as to gaseous pressure on two grounds†:—

1. That an expression for the pressure involving both velocity and density leads to absurd results.

2. That the analytical argument upon which I based such an expression fails, "the question at issue" being "a purely physical one"—the pressure prevailing in a fluid in motion being, as he considered, settled by Boyle's law, which he assumed to hold in that case.

How far the first part of this criticism was justified I remit to the judgment of those who may think it worth while to refer to my papers in the Philosophical Magazine for August and February last how far the second part is consistent with fact appears from the second paragraph of Mr. Strutt's recent paper.

6 New Square, Lincoln's Inn,
June 2, 1873.

NOTE ON THE USE OF A DIFFRACTION-"GRATING" AS A SUBSTITUTE FOR THE TRAIN OF PRISMS IN A SOLAR SPECTROSCOPE. BY PROF. C. A. YOUNG.

Since the diffraction-spectrum differs from a prismatic spectrum of the same length in having the less-refrangible rays more widely dispersed, it some time ago suggested itself that a so-called "*Gitterplatte*," or "grating" of fine lines, might advantageously replace the prisms in spectroscopes designed for the observation of the solar prominences through the C line. In this idea I was strongly confirmed on seeing last winter some of the beautiful gratings ruled upon speculum-metal by Mr. Chapman, Mr. Rutherford's mechanician. The spectra furnished by these plates far exceed in brilliance and definition any thing of the kind ever before obtained.

* See paper "On the Law of Gaseous Pressure" in the Philosophical Magazine for June 1873.

† See paper "On Mr. Moon's Views on Gaseous Pressure" in the Number of the Magazine for July last.

Through the kindness of Mr. Rutherford I have recently come into possession of one of them, having a ruled surface of something more than a square inch, the lines being spaced at intervals of $\frac{1}{6480}$ of an inch. Combining this with the collimator and telescope of a common chemical spectroscope, we get an instrument furnishing a spectrum of the first order, in which the D lines are about twice as widely separated as by the flint-glass prism of 60° belonging to the original instrument. In the neighbourhood of C the dispersion is nearly the same as would be given by four prisms.

The spectra of the higher orders are generally not so well seen, on account of their overlapping each other; but fortunately with one particular adjustment of the angle between the collimator and telescope, the C line in the spectrum of the third order can be made to fall in the vacant space between the spectra of the second and fourth orders, and we thus obtain an available dispersion nearly the same as that of the instrument I am accustomed to use.

On applying the new instrument to the equatorial, I found (under atmospheric conditions by no means favourable, though the best that have presented themselves as yet) that in the first-order spectrum I could easily see the bright chromosphere-lines C, D_3, and F; I could also, though with great difficulty, make out $H\gamma$, (2796 K). On opening the slit the outline of the chromosphere and the forms of the prominences were well seen in the spectra of both the first and third orders, quite as well, I think, as with my ordinary instrument in the same state of the air. The spectra are, of course, fainter; but as this loss of light affects the background upon which the prominences are projected as well as the objects themselves, it does not materially injure their appearance.

The grating is much lighter and easier to manage than a train of prisms; and if similar ruled plates can be furnished by the opticians at reasonable prices and of satisfactory quality, it would seem that for observations upon the chromosphere and prominences they might well supersede prisms.—Silliman's *American Journal*, June 1873.

DUPLEX TELEGRAPHY.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

It may be as well to say that the plan of duplex telegraphy by means of two batteries, working together on the receiving instrument, but counteracting one another on the instrument at the sending end, was patented by me some months since. It is one of the many instances in which a plan occurs to two different people nearly at the same time.

Yours obediently,
H. Highton.

Putney, June 11, 1873.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1873.

X. *Fluorescent Relations of certain solid Hydrocarbons found in Petroleum Distillates.* *By* HENRY MORTON, *Ph.D., President of the Stevens Institute of Technology**.

IN a previous paper embodying the results of some observations upon the fluorescent relations of commercial anthracene†, I have alluded to the existence of an analogous body among the final products of some petroleum-distillations; and I now propose to relate more fully the facts developed in a study of these bodies.

Nearly a year since, Professor E. N. Horsford placed in my hands a small specimen of petroleum-distillate, from which I succeeded in separating a crystalline solid, fluorescing in a remarkable manner with a bright green colour.

The amount of material at my command was too small (being in fact but a few grains, extracted from less than an ounce of the substance sent me) to admit of more than a preliminary examination. I succeeded, however, in establishing its optical (fluorescent and absorptive) relations to commercial anthracene, and its difference from that substance in fusing-point and solubilities.

After encountering some difficulty in tracing up the supply to its source, I was introduced by Professor G. F. Barker to Mr.

* Communicated by the Author.

† See this Journal, 1872, vol. xliv. p. 345.

John Truax, of Pittsburgh, who has been most obliging in furnishing me with every facility that could be wished.

The preliminary history of the material with which we have to work is as follows :—

When the residues left in the distillation of petroleum for the manufacture of illuminating-oils are redistilled to obtain lubricating-oils and paraffine, there passes over near the end of the operation, and when the still is at the bottom almost or even quite red-hot, a thick resinous matter of a dark honey- or light sepia-colour, which is used as a lubricant for the necks of "rolls" in iron mills. This is the material from which the new body is extracted by the following process.

The tarry matter above described is mixed with about its own volume of benzine (petroleum-naphtha), and is thrown on a stout filter, where it is well washed with the same solvent.

This leaves a dark olive-green flaky powder, constituting about 3 per cent. of the original mass, and very similar in appearance to crude commercial anthracene. We now wash this with alcohol, and even digest it in this liquid, by which means a brown material, whose solution in alcohol has a decided blue fluorescence, is removed.

The substance is then dissolved in hot benzole (coal-tar naphtha) and filtered in a jacketed funnel (by which operation a quantity of black powder is removed), and is allowed to crystallize on cooling.

It then deposits in very small acicular crystals collecting in tufts; and the purification is carried on by re-solution and crystallization from fresh benzole. The substance then has the colour of potassium chromate.

The following Table of solubilities will indicate the motive for the above treatment.

Benzine, hot (160° F.),	dissolves	1 part in	1155
„ cold (70° F.)	„	„	2900
Alcohol, hot (160° F.)	„	„	4172
„ cold (70° F.)	„	„	16625
Benzole, hot (160° F.)	„	„	95
„ cold (70° F.)	„	„	152

This substance dissolves in turpentine pretty freely, and yet more so in carbon-bisulphide and chloroform.

In ether and olive-oil and carbon-bichloride it is hardly as soluble as in benzine.

Methods of Examination.

For the study of the spectrum of the fluorescent light, the

following arrangement was made. The porte-lumière A being attached to the shutter of a window facing towards the south, a beam of sunlight was thrown by it horizontally into the room and concentrated by a lens of 12-inch focus placed at B. At C was placed an apparatus (fig. 2) consisting of a circular horizontal table adjustable up and down on a vertical rod and turning with a "click." Around the circumference of this table were eight little "stalls" capable of holding test-tubes or specimen-bottles. By this means eight different specimens could be rapidly compared, each in succession, being brought by the action of the click into an identical position with reference to the exciting light and the spectroscope. This whole apparatus was so placed that an image of the sun was formed on the tube or bottle nearest to the lens B. A glass tank filled with a strong solution of ammonio-cupric sulphate was placed, as indicated, between B and C; and to this was sometimes added a plate of violet glass. The spectroscope D employed in these observations was generally one of Browning's with a single prism of very high dispersion, giving a range of 3° 54′ between C and S of the solar spectrum; but a spectroscope with two similar prisms by the same maker and one by Desaga of less dispersive power were also used occasionally. A small direct-vision spectroscope was also frequently employed to detect the presence of faint lines or bands of absorption.

Fig. 1.

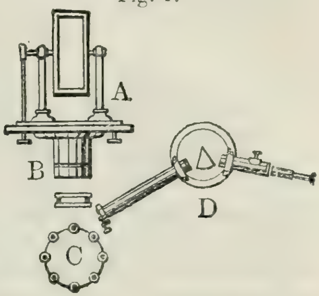
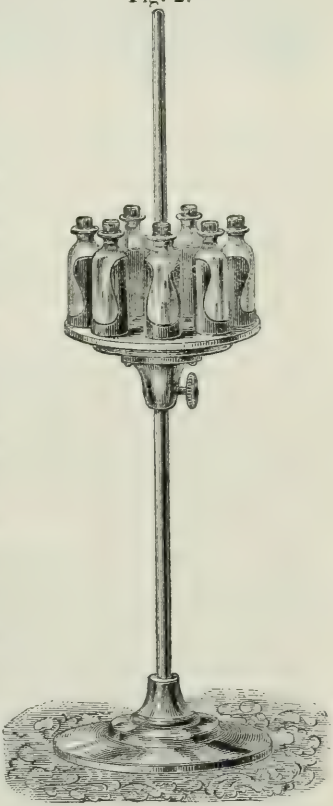


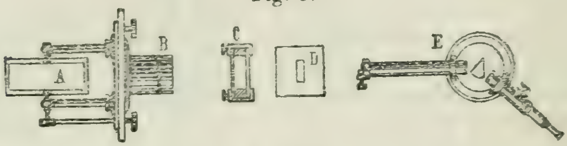
Fig. 2.



For examining absorption-bands, every thing else remained the

same, except that the spectroscope was turned round into the position indicated in fig. 3, and the stand for specimens was replaced

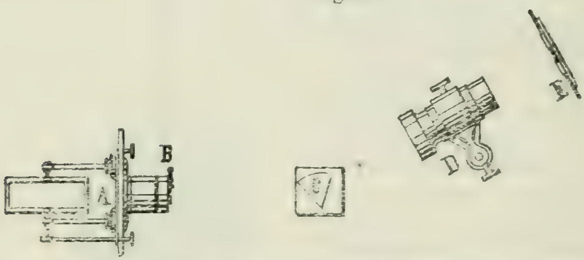
Fig. 3.



by a plane table D, on which the substance to be studied was supported either in a cell of glass or in a bottle. White glass bottles of about one-ounce capacity formed cylindrical lenses of a very convenient character for this purpose.

To examine the relative exciting-power of the various colours of the spectrum, an arrangement, nearly identical with that described by Professor Stokes as his "Third Method"*, was employed.

Fig. 4.



In this case the light thrown in by the porte-lumière A passed through a narrow adjustable slit at B, then encountered a prism at C, and after this a lens of about 12-inches focus at D, by which an image of the spectrum was thrown on a screen or tank at E. A spectroscope placed behind this screen was employed, in a manner presently to be described, to measure the refrangibility of certain rays.

Thus, suppose a pure spectrum showing the Fraunhofer lines clearly to be thrown on a screen coated with some fluorescent body placed at E, and that a point of maximum or minimum fluorescence were observed, and we wished to determine accurately the refrangibility of the rays to which this effect was due.

A fine needle was made to pierce the paper at this point; and the light thus allowed to pass through the screen was then measured by the spectroscope. Of course the hole, once made, could, by movement of the screen, be brought into coincidence with any point in the spectrum.

* Philosophical Transactions, 1852, Part II. p 470.

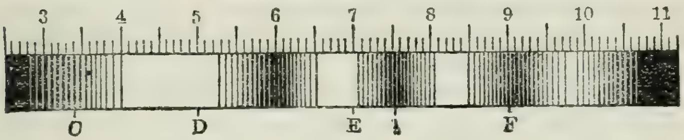
With solutions in tanks the same plan was followed, a small piece of card with a needle-hole being moved into the required position and the transmitted light then measured.

Observations.

Spectrum of Fluorescent Light from Solid Thallene.

When a portion of the yellow crystalline substance above described, and which, to avoid circumlocution, I shall hereafter call thallene, in allusion to the brilliant green colour of its fluorescence, is examined in the manner first described and represented in fig. 1, we obtain a spectrum such as is indicated in fig. 5.

Fig. 5.



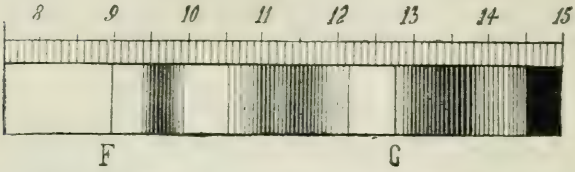
This begins with a very broad bright space in the orange and yellow; then, separated by shades or bands of less brilliancy, follow two green spaces, and lastly one of blue, much less brilliant than the others, and best seen when the violet glass is added to the blue tank (see fig. 1). The scale employed in the above and subsequent drawings of spectra is that with millimetre-divisions introduced by Bunsen.

This spectrum, it will be seen, differs from that of impure anthracene, or chrysogen as found in commercial anthracene, in two respects. First, there is no decided separation of the orange and yellow rays from the red by a dark space in this thallene-spectrum as in the other. Second, there is found in this spectrum a strong bright band in the blue under circumstances in which it cannot be recognized in the spectrum given by the former substance.

Absorption-spectrum of Solid Thallene.

At a temperature of about 460° F., thallene fuses without decomposition; and we may thus obtain a translucent plate of it between slips of mica or on glass in a condition convenient for the study of its absorption in the manner shown in fig. 3. But we may also dissolve it in melted paraffine, or spread it by rubbing on filter-paper, or mix it with varnish and so coat paper with it. In any of these shapes, when examined by transmitted light, as in fig. 2, it shows an absorption-spectrum such as is represented in fig. 6.

Fig. 6.



In this we find a very strong narrow band having the line F in its centre, then a less-defined double band at 100 and 110 of the scale; another, yet less sharply defined, from 120 to 130 connected by a shade with the point about 140, where the absorption becomes total. The marked difference between this and the absorption-spectrum of commercial anthracene consists in the double character of the second band, and in the much lower position of the third band, which ends, above, about where that of the anthracene begins.

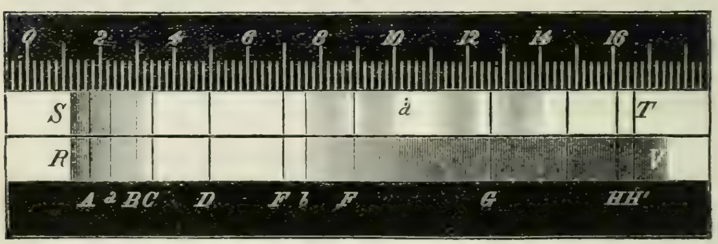
We will now pass to the method of observation indicated already in fig. 4.

The screen upon which the pure solar spectrum was thrown, located at E (see fig. 4), was prepared as follows:—A piece of filter-paper was coated with pure thallene by rubbing it on in powder with the finger. It adheres very well to the paper, and produces in this way a very regular and beautiful surface.

A strip of this is attached with gum to a piece of white card; and this slides in horizontal grooves in a frame E (fig. 3), which is pierced by a large opening.

The spectrum is so adjusted as to fall on the fluorescent strip, and also on the adjacent part of the card; and we have the appearance indicated in fig. 7, where S T represents the part of the spectrum which falls on the thallene-covered paper, and R V that portion which falls on the card. The spectrum upon the card runs, of course, from red to violet, but even below the fixed line G becomes very dim, and between G and H ceases to show any decided light. Where these higher rays fall upon the

Fig. 7.



thallene, however, in place of becoming less brilliant towards the upper end, they glow with remarkable intensity; and all about 14 we find a brilliant green ground on which the lines H, H′ and others appear with beautiful distinctness.

Below this the colour is a bluish white as far as a little below F, beyond which there is no fluorescent action, and the spectrum looks alike whether it fall on the thallene or on the card.

The brightness of the spectrum on the thallene, however, is not uniform between 8 and 14, but presents a series of graduated maxima and minima, which are represented in the woodcut (fig. 7).

To measure the positions of these maxima, the simple device already alluded to was employed. A pinhole was pierced in the fluorescent paper at some convenient point, as at *a* (fig. 7); and by sliding the entire card or screen in the grooves which supported it, this pinhole was brought to the part of the spectrum whose location it was desired to fix (for example, as in the figure, at the middle of the broad maximum *a*); and then, the spectroscope being placed behind E, the refrangibility of the light passing through the pinhole is directly measured.

A comparison of fig. 7 with fig. 6, bearing in mind that the scale of the latter is double that of the former in actual dimensions, though the numbers correspond, will show that the maxima in fig. 7 exactly correspond with the absorption-bands of fig. 6.

This indicates that the absorption in this case is in very close relations with the fluorescence of the substance; and to this conclusion many phenomena to be presently noticed give confirmation. That such a connexion should exist is eminently natural, since those rays which expend their force in fluorescence, *i. e.* in producing vibrations of a less rate, must evidently disappear; but it does not follow that all rays which are absorbed develop fluorescence; and so, as in the uranium salts, we may expect to find absorption-bands which have no immediate relation to fluorescence, although here also there will always be a special absorption corresponding to the maximum of fluorescence-exciting power. This was observed by Stokes at the outset. What we have to notice here is that this body thallene has no appreciable absorption other than that due to its fluorescence—or, in other words, that nearly all the rays absorbed by it are converted into fluorescent motions and not into those of heat.

I have spoken only of the rays as high as H′ when referring to the fluorescence excited by a pure spectrum on a screen of thallene; but it must not be supposed that this is the upper limit of the effect. With lenses and prisms of quartz, I have

projected on such a screen a solar spectrum showing the fixed lines for a great distance above H.

The fluorescence excited by rays about and above H does not, however, seem to suffer any change in intensity, but yields a uniform and brilliant green light, on which the solar lines are seen with admirable distinctness. Indeed this substance seems to me to surpass any heretofore known as a screen for the study of the violet and extra-violet rays.

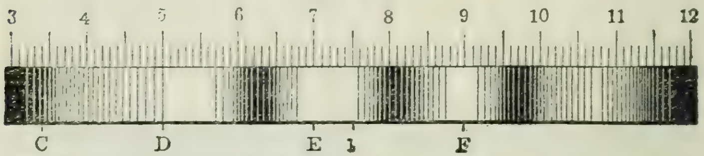
Thallene in Solution.

As had been already noticed, thallene is soluble in a number of liquids; and in all cases it communicates a strong *blue* fluorescence to the solvent, or, more strictly, fluoresces strongly with a blue colour in solution.

When this blue fluorescent light is examined with the spectroscope, it is found, like the green fluorescence of the solid, to break up into bands; but in the solution these are found in all cases to be displaced strongly towards the upper end of the spectrum, which at once explains the change of general tint in the light from green to blue.

Fig. 8, which represents the spectrum of the solution in benzole, will, by comparison with fig. 5, indicate this change; and we shall see that the three most distinct bright bands at 6·8, 8·4, and 9·8 in the spectrum of the solid are moved to 7·2, 8·9, and 10·7 respectively in the spectrum of the solution.

Fig. 8.



If in place of benzole we employ chloroform as a solvent, we find that the bands occupy sensibly the same positions; but if sulpuric ether is used, the bands are somewhat more displaced. With turpentine the displacement seems to be a little less than with ether, and with alcohol about the same, while with olive-oil an intermediate position is occupied, and with carbon-bisulphide the displacement is least of all.

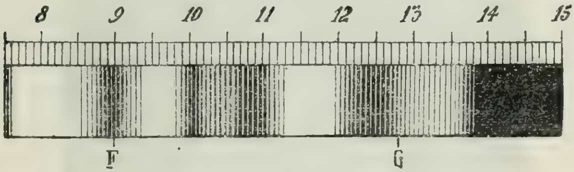
The following Table will give some measurements of the positions of corresponding bands in various solutions:—

Carbon-bisulphide.	Benzole and chloroform.	Olive-oil.	Turpentine.	Ether.
68·0	71·6	72·9	72·9	73·8
87·6	88·5	89·6	89·8	90·1
106·6	107·8	109·2	108·1	

If now we observe the absorptive action of these solutions in the manner indicated in fig. 3, we shall find that the absorption-bands, like those of fluorescence, are displaced upwards in the spectra of the solutions as compared with their position in that of the solid.

This will be illustrated by comparing fig. 9, which represents

Fig. 9.



the absorption-spectrum of thallene dissolved in benzole, with fig. 6, which is the absorption-spectrum of solid thallene.

The absorption-bands of the solutions in benzole, chloroform, and olive-oil do not appear to differ in any important degree; but in the etherial solution the bands are higher than in the others. In the solution in turpentine the lower band especially is exceedingly faint, so as to escape recognition entirely in the method of observation employed in the other cases, even when a pint bottle of solution was used; but when the tube of a Duboscq saccharimeter was filled with this solution, this line could be detected with a direct-vision spectroscope.

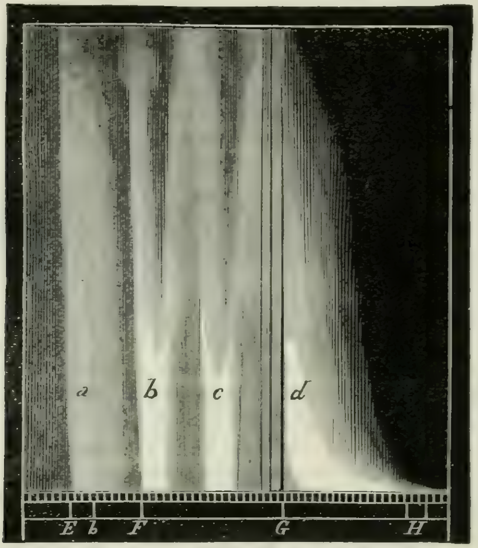
In carbon-bisulphide solution a similar difficulty was encountered, so that in a preliminary publication I spoke of these bands as not appearing; but renewed observation has enabled me to assure myself that in this solution all the bands exist, and at lower points in the spectrum than in any of the other solutions yet examined, the lowest one in particular occupying a point even below that of the corresponding band in solid thallene. There is also in this solution a decided general absorption of all rays above F, which causes some special difficulty in measuring the bands.

In such cases as this the mode of observation to be next noticed is specially efficient as a guide, indicating what we must look for in each case. A similar displacement of bands by change of solvent has been noticed by Hagenbach in the case of soot, amide of phthalic acid, chlorophyl, purpurine, &c. (Poggendorff's *Annalen*, 1872, vol. cxlvi. p. 533); and I have also found it in a number of the uranium salts.

When a pure spectrum is thrown on the side of a tank of glass or quartz filled with the benzole solution, by the arrangement of prism &c. indicated in fig. 4 we see, on looking down

into it from above, long trails of light running through the solution, as shown in fig. 10.

Fig. 10.



These trails are variously coloured as follows: *a* is very faint and olive-green; *b* is very bright and of a vivid green; *c* is bright sky-blue; and *d* is of an indigo tint running into violet towards H.

The appearance is indeed exceedingly beautiful, from the richness of the colours and the delicate manner in which they fade off and blend.

The brightest bands or trails (*b* and *c*) are each penetrated, as it were, from the further side of the tank by a long blade of darkness, the result undoubtedly of the absorptive action of the fluid for the corresponding rays, which causes them to be more rapidly exhausted; while those at either side, being less energetically active and acted upon, penetrate further before their motion is converted into fluorescence. Near H, where the actions of absorption and fluorescence are most intense, the light hardly seems to pass beyond the actual surface of the liquid.

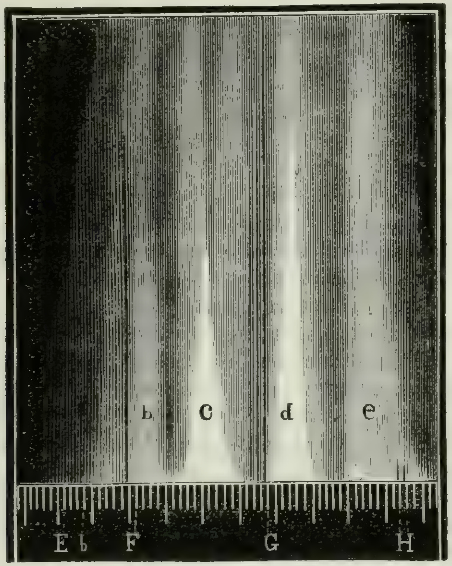
Exact parallels to all these actions were observed by Stokes in chlorophyl (Phil. Trans. 1852, Part II. p. 489); but though I have examined all the substances enumerated by him, I have in no case found so beautiful an exhibition of the phenomena as this solution furnishes.

When the light from these trails is examined with a hand-spectroscope, it is seen that the bright green one *b* wants the

upper blue band 10·7, which is present in *c* and *d*. This was a necessary result from Stokes's law, that fluorescent light never contained rays of higher refrangibility than that by which it was excited; for the exciting light of *b* was only a little above F, or 9 to 9·15 of the scale.

Turpentine dissolves only a small portion of thallene, which, however, communicates to it a blue fluorescence. When examined in the manner just described, its appearance resembles in many respects that of a similarly weak solution in benzole; but there are certain points of distinction. Thus the first band above F is at some distance from it; the second band is decidedly double, with its upper half of a paler colour; and the band above G is likewise evidently divided (see fig. 11).

Fig: 11.



The dark blades of absorption are not seen in this or in a dilute benzole solution.

A solution of thallene in carbon-bisulphide having, as we have seen, its absorption-bands very low in the spectrum, produces also in this last method of examination a very characteristic appearance.

The first trail of light is entirely below F, and is pierced by a dark blade; its colour is bright green. The next is midway between F and G, is bright blue, but does not penetrate very far into the solution, and does not show a black blade. The upper

trail is of a faint indigo tint, penetrates but little into the liquid, and begins decidedly below G.

In a former paper attention was drawn to the effect which an exposure to sunlight produced upon the bands of absorption exhibited by commercial anthracene. A cold solution of that substance, showing its bands with great distinctness, loses them entirely by an exposure of ten minutes to the direct rays of the sun. Thallene similarly treated loses its lower band in five minutes, but its upper one (*i. e.* the broad double band) only after thirty minutes' exposure.

A saturated hot solution under like conditions loses its lower band in thirty minutes, and its upper one in about two hours. By placing it, however, near the focus of a large burning-glass of 15 inches diameter, ten minutes sufficed for this effect. When a solution of anthracene so exposed for one minute is allowed to cool, it deposits white pearly scales (of pure anthracene), which show no trace of a banded spectrum by fluorescence.

When a solution of the yellow anthracene (obtained by washing the crude material with benzine, dissolving in hot benzole, filtering, and crystallizing by cooling), which contains much chrysogen, is similarly treated, after ten minutes' exposure in the focus of the lens, it deposits on cooling crystals of anthracene nearly white, but still showing a banded spectrum; the bands are weaker than before, but occupy exactly the same positions; the general fluorescence of the substance is also greatly reduced.

Thallene, however, under similar conditions (that is to say, exposed in hot solution in a 4-ounce flask for ten minutes near the focus of the large lens and allowed to cool) deposits crystals almost white but having a slight grey shade; and these show a distinctly banded spectrum, whose bands are located in almost the same positions as those of an etherial solution, but are a little more elevated.

Their centres are in fact located as follows:—7·34, 9·13, 11·17, omitting the lowest band, which is too broad to be well indicated in this way.

The general colour of the fluorescent light emitted by this substance is light blue; and to distinguish it I would propose the name Petrollucene.

A solution of this in benzole shows a slight further elevation of bands, as 7·38, 9·2, and 11·24; and when a pure spectrum is thrown on this solution only two trails of light are seen—one about four fifths of the way from F to G, and the other above G. These are blue and indigo in colour respectively.

As illustrations of the action of fluorescence, these substances (thallene and petrollucene) surpass, I think, any bodies heretofore known.

The green light emitted by thallene surpasses in intensity that furnished by any other substance, the only one which approaches it being platinocyanide of barium in a peculiar condition, which, however, is by no means under control.

There are in the collections of the Stevens Institute of Technology some screens and words painted with this salt on cards of a few inches area, prepared by Albert Sohn, of Frankfort-on-the-Maine, which approach thallene in brilliancy; but the pure salt, even when forming fine crystals, is very inferior in the energy of its fluorescence.

Anthracene forms, as is well known, a compound with chlorine (bichloranthracene), whose solutions in alcohol and benzole fluoresce brightly and give a spectrum (banded in the upper regions) which has been studied by Hagenbach (Poggendorff's *Annalen,* vol. cxlvi. p. 385.

This led me to attempt the preparation of a like compound with thallene.

A quantity of bichloranthracene having been first made, to ensure accuracy in the method of working, thallene was submitted to the same treatment, *i. e.* was mixed with benzole and subjected to a current of chlorine for about an hour, and was again exposed in the dry state to the action of chlorine. A strong reaction was shown, considerable heat being developed, and the entire substance acquiring a chocolate colour. The product, however, showed no tendency to crystallize. In its tarry state and in solution its fluorescence is moderately strong, but yields a continuous spectrum from about C to E.

Having next prepared some bibromanthracene and examined its fluorescence, I submitted thallene to an identical treatment. But here also, though a compound was formed crystallizing in very minute needles from its hot benzole solution, neither the solid nor the solution showed any indications of fluorescence.

I found moreover that, while anthracene combines with about twice its weight of picric acid to form the picrate, which crystallizes in long needles of a rich strawberry colour, thallene requires fully four times its weight of picric acid to saturate it. On cooling from hot solution, a granular mass of minute crystals is formed, having a rich orange-red colour. Examined under the microscope, these crystals are seen to be short prisms with well-defined pyramidal terminations.

Sulphuric acid of density 60° B., which rapidly blackens anthracene even at the ordinary temperature, gives thallene a rich grass-green colour, which remains unchanged even after standing for weeks and exposure to a temperature of 60° C.

In this condition it continues to give a bright fluorescence,

yielding a banded spectrum identical with that characterizing it in its normal state.

I should therefore conclude that no true combination had occurred, and that the change from a yellow to a green colour resulted from the presence of minute black particles produced by the action of the sulphuric acid upon some small portion of an adherent impurity. This view is confirmed by the fact that with petrollucene, which is almost colourless, sulphuric acid produces a dark green colour, approaching to black. Continued application of heat causes thallene, treated as above, to dissolve, forming a compound perfectly soluble in water.

The picric-acid compounds, both of anthracene and thallene, show no fluorescence; so that this gives us a convenient method of determining the point of saturation.

Examined with Becquerel's phosphoroscope, the duration of fluorescence in solid thallene was found to be about $\frac{1}{500}$ of a second; but that of its solution was too brief to be appreciable even at the highest velocity which could be given to the instrument.

To exhibit the fluorescence of thallene, I have found the following methods most effective:—

1st. The electric light, without any lenses, is allowed to fall directly on it through a plate of blue or violet glass. In this way one can show various devices, filling an entire stage without difficulty. Allowing the light to pass through the condensing-lenses of a lantern greatly reduces the effect.

2nd. Burning magnesium wire behind a blue glass. The effect here also is very powerful.

3rd. Connecting in series a number of nitrogen-spectrum tubes and exciting them with a powerful coil. The great lack of general illuminating-power in the light so produced makes the effect in this case very striking.

A lime-light behind a blue glass will exhibit the fluorescence of thallene fairly well, but not that of petrollucene.

The violet end of a projected spectrum might be regarded as a good source of exciting light, but is, as a matter of fact, inferior to any of those before mentioned. Nevertheless the otherwise invisible bands of metallic spectra, up to the position of the group O, may be exhibited to the largest audiences by the use of a screen covered with thallene. In painting designs and screens with thallene, it is best to grind it with damar varnish and benzole. A glass goblet coated with thallene is well suited for Gassiot's electric cascade.

XI. *On the Form of the Cells of Bees.* *By* J. W. L. Glaisher, *B.A., Fellow of Trinity College, Cambridge**.

THE following account of the history of the problem of the bee-cell is extracted from pp. 428–430 of 'Homes without Hands,' by the Rev. J. G. Wood (London, 1869):—

"If a single cell be isolated, it will be seen that the sides rise from the outer edges of the three lozenges above mentioned; so that there are, of course, six sides, the transverse section of which gives a perfect hexagon. Many years ago Maraldi, being struck with the fact that the lozenge-shaped plates always had the same angles, took the trouble to measure them, and found that in each lozenge the large angles measured 109° 28′, and the smaller 70° 32′, the two together making 180°, the equivalent of two right angles. He also noted the fact that the apex of the three-sided cup was formed by the union of three of the greater angles.

"Some time afterwards, Réaumur, thinking that this remarkable uniformity of angle might have some connexion with the wonderful economy of space which is observable in the bee-comb, hit upon a very ingenious plan. Without mentioning his reasons for the question, he asked Kœnig, the mathematician, to make the following calculation:—Given a hexagonal vessel terminated by three lozenge-shaped plates, what are the angles which would give the greatest amount of space with the least amount of material? Kœnig made his calculations, and found that the angles were 109° 26′ and 70° 34′, almost precisely agreeing with the measurements of Maraldi. The reader is requested to remember these angles. Réaumur, on receiving the answer, concluded that the bee had very nearly solved the difficult mathematical problem, the difference between the measurement and the calculation being so small as to be practically negatived in the actual construction of so small an object as the bee-cell.

"Mathematicians were naturally delighted with the result of the investigation; for it showed how beautifully practical science could be aided by theoretical knowledge; and the construction of the bee-cell became a famous problem in the economy of nature. In comparison with the honey which the cell is intended to contain, the wax is a rare and costly substance, secreted in very small quantities, and requiring much time for its production; it is therefore essential that the quantity of wax employed in making the comb should be as little, and that of the honey contained in it as great as possible.

"For a long time these statements remained uncontroverted.

* Communicated by the Author.

Any one with the proper instruments could measure the angles for himself; and the calculations of a mathematician like Kœnig would hardly be questioned. However, Maclaurin, the well-known Scotch mathematician, was not satisfied. The two results very nearly tallied with each other, but not quite; and he felt that in a mathematical question precision was a necessity. So he tried the whole question himself, and found Maraldi's measurements correct, namely 109° 28′ and 70° 32′.

"He then set to work at the problem which was worked out by Kœnig, and found that the true theoretical angles were 109° 28′ and 70° 32′, precisely corresponding with the actual measurement of the bee-cell.

"Another question now arose. How did this discrepancy occur? How could so excellent a mathematician as Kœnig make so grave a mistake? On investigation it was found that no blame attached to Kœnig, but that the error lay in the book of logarithms which he used. Thus a mistake in a mathematical work was accidentally discovered by measuring the angles of a bee-cell—*a mistake sufficiently great to have caused the loss of a ship whose captain happened to use a copy of the same logarithmic tables for calculating his longitude.*"

It is proper to add that Mr. Wood prefaces the above narrative with the words, "I must acknowledge my thanks to the Rev. Walter Mitchell, Vicar and Hospitaller of St. Bartholomew's Hospital, who has long exercised his well-known mathematical powers on this subject, and has kindly supplied me with the outline of the present history."

The last sentence in the first extract, viz. that about the logarithms (in which the italics are Mr. Wood's), induced me to examine the original memoirs that relate to the history of the problem of the form of the bee-cell; and although the story as given above is on the face of it erroneous, as the determination of the angles that occur in a bee-cell by measurement to the nearest *minute* is clearly impossible, still I was not prepared to find that the whole story narrowly escapes the verdict *tot sententiæ, quot mendacia**.

As, however, the manner in which so elaborate and romantic a fable has gradually grown up out of simple and even commonplace facts possesses interest, and as moreover most of the misstatements have received the sanction of previous writers, I think

* It is only fair so far to anticipate matters as to remark at once that Mr. Wood (or Mr. Mitchell) is not chargeable with the origination of the false statements that occur in the narrative quoted, for most of which in the above shape Lord Brougham seems to be chiefly responsible; but the concluding paragraph about the logarithms (which will be seen to be totally without foundation), as far as I know, first makes its appearance in 'Homes without Hands.'

it will be found that the history of the matter is well deserving of attention, although the account quoted, occurring in a work of a popular character, would not, however glaring its inaccuracies, *per se* require refutation in a scientific journal.

It is convenient to give at the outset, before entering into any details, a brief account of the facts that did occur, for comparison with that quoted above. Maraldi measured the angles of the rhombs and also those of the trapeziums*, and found that, as nearly as he could determine them, the larger angles in both cases were about 110°, and the smaller the supplement of this angle, viz. 70°; it naturally occurred to him to investigate mathematically what the angles must be on the supposition that those of the rhombs and trapeziums were exactly equal, and he found as a consequence of this hypothesis that the values in question were 109° 28′ and 70° 32′. These angles were not, therefore, the results of direct measurement, but were derived from the solution of a mathematical problem suggested by the actual measurements. Réaumur, suspecting that economy of wax was the reason for the angles being such as they were, explained the form of the cell to Kœnig, and (without mentioning what had been done by Maraldi) asked him to determine mathematically what the angles of the rhombs would be if for a given capacity the surface of the cell were made a minimum. Kœnig solved the problem, and gave as his result 109° 26′ and 70° 34′, differing by only 2′ from what Réaumur (erroneously) regarded as the angles absolutely measured by Maraldi. Kœnig accompanied his solution by some remarks to the effect that the question was one that required the methods of Newton and Leibnitz, so that the bees had solved a problem that was beyond the reach of the old geometry. Maclaurin showed that this was inaccurate, by giving a solution depending only on ordinary geometry; and he found the angles to be 109° 28′ and 70° 32′, agreeing with Maraldi's theoretical values. Maclaurin read Maraldi's memoir correctly, and understood the angles by measurement to be 110° and 70°; and he merely just notices the trifling error made by Kœnig in his calculation, regarding it as quite an unimportant matter.

These are all the facts on which Mr. Wood's narrative is founded; and it therefore follows (1) that Maraldi did not find the angles by measurement to be 109° 28′ and 70° 32′, (2) that Maclaurin was not led to consider the matter by observing the discrepancy between Maraldi's and Kœnig's values, (3) that he did not make any measurements himself, and (4) that the dis-

* By the angles of the trapeziums in this paper are invariably meant the two angles where the sides of the cell are intersected by the rhombs; the angles at the mouth of the cell are, of course, right angles.

covery of the error in the logarithmic table is a fiction, the method Kœnig employed never having transpired, so that it is quite as likely as not that he never used logarithms at all.

The sentence about the pleasure of the mathematicians is no doubt merely rhetorical; but few persons who have any knowledge at all of the history of science will be able to read without a smile the grave statement that, more than half a century after the publication of the *Principia*, mathematicians were *delighted* at their science having given a proof of its utility by the solution of a simple geometrical question.

Before entering into further detail with regard to the history of the problem of the bee-cell, which presents several other points besides those already mentioned, it is proper to state that the subject was treated, in very much the same way as in this paper, by the late Robert Leslie Ellis, in a short memoir which was first published after his death in the volume of his collected writings (edited by Walton, Cambridge, 1863), pp. 353–357. Ellis's remarks were called forth by Lord Brougham's essay on the subject (which will be noticed further on and seen to be very inaccurate); and he has pointed out that the values 109° 28′ and 70° 32′ were not the result of measurement; he has also corrected Lord Brougham on another point (viz. about Boscovich), which will be referred to presently. The former misconception Ellis speaks of as being "still current and repeated by every writer or nearly so on the subject;" and he refers to a further inaccuracy occurring in Dr. Carpenter's 'Physiology'*. As, however, Ellis's paper occupies only a small portion of the ground covered by the present communication, it will be convenient for the sake of completeness to give the whole account here as if it had not been written; but it must be understood that the portion relating to Maraldi is exhaustively treated by him†. Maraldi's memoir is entitled "Observations sur les Abeilles," and occupies pp. 299–334 of the Memoirs of the French Academy for 1712 (Paris, 1714)‡. The result of the measurements is given on p. 309 in the following words:—

* The passage in question ('Human Physiology,' 3rd edit. 1856, p. 251) concludes with the words, "the *bees* being thus proved to be *right* and the *mathematician wrong*." Dr. Carpenter refers to Lord Brougham (*vide infrà*), from whose essay he takes the account, and who is responsible for all except the statement that he (Lord Brougham) took into account certain small quantities that had been previously neglected, and thus showed that the agreement of theory with observation was perfect.

† It should be mentioned that Ellis adds some remarks on the construction of the cells which are not connected with the subject of the present paper.

‡ But there are different editions; in another edition (Paris, 1731 before me the memoir occupies pp. 297–331, and the spelling and accentuation are more modernized; the quotations are from pp. 307 and 309.

"Chaque base d'Alveole est formée par trois rombes presque toûjours égaux et semblables, qui suivant les mesures que nous avons prises, ont les deux angles obtus chacun de 110 degrés, et par consequent les deux aigus chacun de 70 degrés;" and on p. 312 the result of the equality of the angles of the rhombs and trapeziums is given: "Outre ces avantages qui viennent du côté de la figure de la base, il y en a encore qui dépendent de la quantité des angles des rombes; c'est de leur grandeur que dépend celle des angles dez trapezes, qui forment les six côtéz de l'Alveole; or on trouve que les angles aigus des rombes, étant de 70 degrés 32 minutes et les obtus de 109° 28 minutes, ceux des trapezes qui leur sont contigus, doivent être aussi de la même grandeur." The *étant,* as Ellis remarks, being the prothesis of a hypothetical proposition, *if* they are &c. *then* so and so; and in this way the mathematicians Maclaurin, Boscovich, and Lhuillier have read it. Maraldi was an able mathematician; and no doubt, after his previous statement of what his measurements gave, it no more occurred to him that a non-mathematical reader might imagine these angles were the result of more accurate measurements, than it did that there was any necessity to give the details of the solution of the geometrical problem involved.

Réaumur's researches are to be found on pp. 389–391 of t. v. of his *Mémoires pour servir à l'histoire des Insectes* (Paris, 1740). The following is the account of the proposition of the question to Kœnig:—"Convaincu que les abeilles employent le fond pyramidal qui mérite d'être préféré, j'ai soupçonné que la raison, ou une des raisons qui les avoit décidées, étoit l'épargne de la cire; qu'entre les cellules de même capacité et à fond pyramidal, celle qui pouvoit être faite avec moins de matiére ou de cire, étoit celle dont chaque rhombe avoit deux angles, chacun d'environ 110 degrès, et deux chacun d'environ 70. Sans parler de la grandeur de ces angles, après avoir fait admirer la disposition des rhombes à M. Kœnig . . . . je lui proposai de résoudre le probleme suivant. Entre toutes les cellules exagonales à fond pyramidal, composé de trois rhombes semblables et égaux, déterminer celle qui peut être construite avec le moins de matiére." Kœnig gave 109° 26′ and 70° 34′ as the angles of the rhombs; and then Réaumur sent him the 'Memoirs of the Academy' for 1712, and he (Kœnig) "was agreeably surprised" to find therefrom that his results only differed by 2′ from Maraldi's 109° 28′ and 70° 32′, which Réaumur speaks of as "les mesures les plus précises de ces angles." Réaumur unfortunately does not give Kœnig's investigation; but he mentions that it was presented to the Academy in 1739. The volume for this year contains no memoir by Kœnig (so that, if he presented one, it was not con-

sidered deserving of publication *in extenso*); but the matter is noticed at length in pp. 30–35 of the *Histoire* at the beginning, where, however, only a general account of the matter is given, and no further information about Kœnig's solution is added*. The historian of the Academy states that Réaumur also proposed the question to other mathematicians, from whom, however, he did not receive solutions; he speaks of the bees being only in error to the trifling amount of 2′, and adds that the "grande merveille" is their solution of a problem belonging to the higher geometry.

Maraldi proposed to himself the problem, Supposing the rhombs and trapeziums have equal angles, what are they? and Réaumur proposed to Kœnig the problem, What must the angles of the rhombs be, if the surface is a minimum? The answer to the latter question is, "Equal to the angles of the trapeziums;" so that both problems give the same values for the angles; only Maraldi worked his numerical calculation correctly, Kœnig incorrectly. What happened was that one mathematician was right and another wrong, not that the bees were right and the mathematicians wrong. How Kœnig, who had himself just been considering the mathematical problem of the bee-cell, could on reading Maraldi's memoir have failed to see that the latter's values were the result of a geometrical investigation of the same nature as that which he had just completed, and that they ought to have agreed exactly with his own, is perfectly unaccountable; but it seems to have been the fact.

Réaumur (t. v. p. 390) gives, on Kœnig's authority, the amount of wax saved as equal to the whole quantity that would be required for a hexagonal bottom ("M. Kœnig a pourtant démontré que les abeilles œconomisent la cire, en préférant les fonds pyramidaux aux fonds plats, qu'elles ménagent en entier la quantité de cire qui seroit nécessaire pour un fond plat"). This is incorrect; it will appear further on that the saving is between one fifth and one sixth of the amount here stated.

Maclaurin's paper, "Of the Bases of the Cells wherein the

* Réaumur's words (t. v. p. 390) are: "Si je ne craignois qu'on se lassât de m'entendre parler géométrie, je rapporterois volontiers les démonstrations de M. Kœnig; mais ceux qui sont curieux de les voir, n'y perdront rien pour ne les pas trouver ici. Le mémoire qui les donne, a été lû à l'Académie en 1739, il en sera fait mention dans l'Histoire de cette même année; elles y seront exposées plus nettement, et mises dans un plus grand jour, par notre Célébre Historien, que je ne le pourrois faire." This implies that Kœnig's solution was presented *in extenso* to the Academy, and that Réaumur expected it would be so published. In point of fact, however, as already mentioned, not one word of the solution itself was printed; and all that can be inferred about it from Réaumur's and the historian's remarks (which latter are not remarkable for their profundity) is, that it depended on the differential calculus.

Bees deposit their Honey," appears in the Philosophical Transactions for 1743, pp. 565–571*. After an accurate† history of the question, he gives a geometrical proof of Kœnig's proposition to disprove the assertion that any "higher geometry than was known to the Antients" was necessary, and correctly finds the angles to be 109° 28′ 16″ and 72° 31′ 44″. He proceeds, "Though Monsieur Maraldi had found by his mensuration these obtuse angles to be of about 110 degrees, the small difference between this and the 109° 28′ 16″, just found by calculation, seems to have been either accidental, or owing to the difficulty of measuring such angles with exactness. Besides that he seems to admit the real equality of the several plane angles, that form as well the apex, as the other solid ones we have been treating of. And, as to the small difference between our angle and that determined by Mr. Koënig, who first considered this problem, but has not yet published his demonstration of it, that can only be owing to his not carrying on his computation so far, and would scarcely have been worth the mentioning, were it not yet in favour of the practice of these industrious little insects."

The whole of the above passage has been quoted, because otherwise the concluding words "were it not in favour &c." might give an erroneous impression of Maclaurin's meaning, which from the context is seen to be that the correct answer is 2′ 16″ nearer to the observed angle (110°) than was Kœnig's angle, a correction favourable to the bees. To a not very careful reader the words might at first sight seem to imply that the writer had vindicated the bees from their supposed error of two minutes. Maclaurin calculates the saving as "almost one fourth part $\left[\frac{\sqrt{3}-\sqrt{2}}{\sqrt{2}}\right]$ of the pains and expense of wax they bestow above what what was necessary for completing the parallel sides of the cells." This is quite correct; the amount mentioned is equal to $\frac{\sqrt{3}-\sqrt{2}}{\sqrt{3}} = \cdot 183\ldots$ of the wax required for a plane hexagonal floor. It is strange that Maclaurin did not so express it, and thus point out the important error made by Kœnig in stating it as equal to the whole floor.

Boscovich's investigations occur in the notes to Stay's poem‡.

* The paper appears in full also in the Abridgement by Hutton, Shaw, and Pearson, vol. viii. pp. 709–713 (1809).

† "Monsieur Maraldi had found by mensuration that the obtuse angles of the rhombus's were of 110 degrees nearly; upon which he observed that if the three obtuse angles which formed the solid angles above mentioned were supposed equal to each other, they must each be 109° 28′; from whence it has been inferred that this last was really the true and just measure of them" (p. 566).

‡ Philosophiæ recentioris a Benedicto Stay . . . . versibus traditæ

In the space of six pages and a quarter he has treated the matter very ably and in a manner worthy of his high reputation. He has clearly read with care all that had been previously published on the question, except Maclaurin's memoir, which he knew of and had in vain endeavoured to procure; he therefore does not know what method Maclaurin used, or whether any of his own remarks had been anticipated by him; but he thinks it likely that such may have happened, "cum in tanto Geometra nihil desiderari unquam possit, quod aut ad simplicitatem solutionum, aut ad penetrationem, et combinationes consectariorum pertineat," a well-deserved compliment to the Scotch mathematician.

Boscovich points out distinctly that Maraldi's values 109° 28′ and 70° 32′ were not the result of measurement; and he even goes further and expresses a doubt whether, without the knowledge of the solution of the mathematical problem, he would even have been able to measure them to the nearest degree. His own words are worth quoting:—"Mirum sane esset, si Maraldus ex observatione, angulum æstimasset intra minuta, quod in tam exigua mole fieri utique non poterat. At is ut satis patet ex ipsa ejus dissertatione, affirmat se invenisse angulos circiter graduum 110 et 70, nec minuta eruit ex observatione, sed ex æqualitate angulorum pertinentium ad rhombos, et ad trapezia; ad quam habendam Geometria ipsum docuit requiri illa minuta. Quin immo satis vereor, ne hæc ipsa, aut aliqua ejusmodi consideratio dederit ipsum graduum numerum illum tam proximum; nam observatio intra plures gradus incerta esse debuit in mole adeo exigua, nec ita accuratè efformata; cum plures irregularitates, et in hisce rhombis, et in omni reliqua constructione alveolorum observari passim affirmet ipse Reaumurius.

"Verum quidquid sit de graduum numero, certe numerum minutorum Maraldus non proponit tanquam observatione immediata definitum, sed ex illa æqualitatis consideratione, quam diximus, adeoque si Koenigii solutio rite procederet, determinatio minimi non ab angulo observato per illa duo minuta discederet, sed ab angulo requisito ad ejusmodi æqualitatem, cum observato tamen congrueret adhuc intra limites admodum arctos, et qui in ejusmodi argumento omnem superant observatoris industriam."

Réaumur himself (t. v. p. 382) says that some of the rhombs appear square, but that generally they are "lozanges ou rhombes plus ou moins allongés;" and any one who remarks how greatly

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libri x. cum adnotationibus, et supplementis P. Rogerii Josephi Boscovich, S.J. vol. ii. Rome, 1760, 8vo. The note *De Apium Cellulis* refers to verse 2306 of book vi., and is the last note in the second volume (pp. 498–504). The first volume was published in 1755, and, according to the *Biographie Universelle* (Michaud), the third in 1792, the publication having been retarded by Boscovich's delay in the preparation of the notes.

they differ from one another will agree with Boscovich in his surprise that Maraldi should have been able to obtain by measurement the typical angle correctly to within one ninetieth of a right angle. Of course Boscovich does not impute the slightest insincerity to Maraldi; he merely points out that the knowledge of the solution of the theoretical question may have exercised an unconscious bias over his mind, so as to make him announce 110° with more confidence than he might otherwise have felt.

Boscovich gives two solutions of Kœnig's problems, the one by pure geometry, the other by differential calculus. The latter is obtained in exactly the way any one to whom the question was proposed would probably now proceed (at all events it is nearly identical with my own solution undertaken before I had seen any of the investigations on the matter): the correct result is obtained; and it is suggested that Kœnig's error was produced by his having adopted some complicated method leading to a formula which had to be solved by approximation. With regard to the saving of the wax the process is quite correct; only there is a slip in the course of the work which renders the result inaccurate: the ratio of the amount saved to that required to form a hexagonal base is given as $\frac{\sqrt{6}-2}{2}$, whereas it should be $\frac{3-\sqrt{6}}{3}$. The ratio in question is truly found to be $\frac{2\sqrt{3}-2\sqrt{2}}{2\sqrt{3}}$; but in the next line this is by a mistake written $\frac{\sqrt{3}-\sqrt{2}}{\sqrt{2}}$, and the error is retained. This upsets an ingenious suggestion of Boscovich's, who thought that perhaps Kœnig's error was introduced by the accidental substitution of $\sqrt{6-2}$ for $\sqrt{6}-2$ in the numerator of the fraction quoted, which would change the ratio into one of equality*.

One point of very considerable importance in the form of the cell is first noticed by Boscovich, who observes that by the arrangement adopted every plane cuts every other plane at an angle of 120°: thus the three rhombs forming the apex are in-

* It is scarcely worth speculating how it was that Kœnig did make the error in determining the ratio; and, of course, the facts can never be known with certainty; but the following is a guess. Taking the length of a side of the hexagonal base as unity, the amount of the saving is $\frac{3\sqrt{2}}{4}+\frac{3\sqrt{3}}{2}-\frac{9\sqrt{2}}{4}$ (the first term being the area of the six triangles, the second of the hexagonal base, and the third of the three rhombs); if, then, Kœnig forgot to multiply the area of a single rhomb, viz. $\frac{3\sqrt{2}}{4}$, by 3, the expression would reduce to the middle term, viz. the area of the hexagonal base.

clined to one another at this angle, and so is each trapezium to the planes it cuts. This leads to the belief that the bees have some means (instrumenta) of constructing planes inclined to one another at this angle. Ellis (who had not seen what Boscovich had written) practically remarked the same thing, and made a guess at what the *instrumenta* were a century later ('Writings,' p. 356). It thus appears that Boscovich discussed the whole subject with completeness, penetration, and (but for the one trivial slip) accuracy. Had his remarks been published in a work better known and more accessible to naturalists, a detailed refutation of Réaumur and Kœnig a hundred and thirteen years later would have been rendered superfluous.

The next memoir* that I know of in which the form of the bee-cell is considered is by Lhuillier, and occupies pp. 277–300 of the Berlin Memoirs for 1781 (published 1783). The introduction (of two pages) and conclusion were written by Castillon, who presented the memoir to the Academy. Lhuillier gives a short account of the history of the subject without adverting particularly to Réaumur's misreading of Maraldi. He argues with Boscovich "que l'égalité supposée des angles des rhombes du fond et de ceux des trapezes des faces d'un alvéole, est le principe qui a guidé M. Maraldi dans l'estimation de ces angles;" and adds a new fact, viz. that Cramer, his fellow-citizen (of Geneva) had given some developments on the subject to Kœnig which had not been published. After stating that one of Boscovich's methods is very similar to Maclaurin's, he begins the next paragraph: "Tous les mathématiciens ont regardé cette matière comme passant les forces de la Géométrie élémentaire," which is untrue, as the solutions in question are geometrical: the error is a curious one for Lhuillier to have made, as he had certainly read Boscovich's remarks, and in all probability seen Maclaurin's paper. He very properly regards the difference of method as of very slight importance in a mathematical point of view (and thereby he shows, I think, a truer appreciation of the relations between the modern and ancient methods than either Kœnig or the historian of the French Academy); and he justifies his own geometrical solution that follows on the ground that it is thus made intelligible to naturalists; for although it is unlikely that the same person should be both an able mathematician and an able naturalist, still a particular study of the one does not prevent a knowledge of the elements of the other. After his geometrical solution he proceeds to calculate the saving of wax; and he finds that the amount used by the bee in the real cell is to the amount that would be required for

* "Mémoire sur le minimum de cire des alvéoles des Abeilles et en particulier sur un minimum minimorum relatif à cette matière."

the prismatic cell with a plane hexagonal floor as $25+\sqrt{6}$ to 28, so that the wax saved is about the one fifth part of that required for the plane floor. Lhuillier merely proves the above value for the ratio; but it is interesting to have the amount of wax required for the different portions of the cells ready for comparison. Taking, therefore, a side of the hexagonal section as unity, and assuming with Lhuillier (after Maraldi's observations) that the ratio of the radius of the inscribed circle of the hexagon to the depth of the prismatic cell of the same capacity as the real cell is as $1\frac{1}{5}$ to 5, we have for this depth (viz. the longest side of the trapeziums in the real cell) the value $\frac{25\sqrt{3}}{12}$, and then to three places of decimals—

Area of the three rhombs . . . $=\frac{9\sqrt{2}}{4} = 3{\cdot}182$, (i)

„ six triangles . . . . $=\frac{3\sqrt{2}}{4} = 1{\cdot}061$, (ii)

„ six sides of equivalent prismatic cell . . $=\frac{25\sqrt{3}}{2} = 21{\cdot}651$, (iii)

„ hexagonal base . . . $=\frac{3\sqrt{3}}{2} = 2{\cdot}598$, (iv)

and the surface of the real cell $=$ (i) $+$ (iii) $-$ (ii) $= 23{\cdot}772$, while that of the prismatic cell with a flat hexagonal bottom $=$ (iii) $+$ (iv) $= 24{\cdot}249$; so that the saving is $\cdot 477$, and the ratio is as stated above.

It forms no part of my present purpose to give a detailed account of the form of Lhuillier's *minimum minimorum* cell; suffice it to say that he extends Réaumur's problem, and proceeds to inquire what must be the proportion of the depth of the cell to the width of its mouth that it may require the minimum of wax (the *shape* of the cell, viz. the inclination of the rhombs to the trapeziums and to one another, being supposed as in the bee-cell). A few remarks on this paper will be made further on.

Lhuillier refers to Lambert (*Beyträge zum Gebrauche der Mathematik,* t. iii. p. 387 *et seq.* Berlin, 1772); but I find on reference there is nothing that very closely relates to the subject of this paper. Lambert's remarks occur in the course of an essay on the Art of Building; and he prefaces chap. v. (on minima in roofs) with the statement that if it were usual to build hexagonal houses we should obtain our model from the bees. In point of fact hexagonal summer-houses, sentry-boxes, towers, &c. are not very uncommon; so that he investigates the condition of roofs of minimum surface both in this case and when the base is square. As the result of the former problem he obtains

the bee-cell; and when the base is square the roof is formed of four rhombs, which happen to be of the same form as in the bee-cell (viz. ratio of diagonals $= \sqrt{2}$), with the only difference that when the base is square they are placed with the shorter diagonals horizontal, and when the base is hexagonal the longer. He thence concludes that with twelve such rhombs a regular figure of twelve sides could be made exhibiting both the roofs of the 4- and 6-angled buildings, and therefore also of the bee-cell; after which he adds that this last fact was noted by Kepler in his *Harmonice*, so that the question Réaumur proposed to Kœnig was not so new as he thought (nicht mehr so ganz neu, als es Reaumur geglaubt zu haben scheint). Lhuillier says Lambert's assertion about the bee-cell is contrary to the contents of his (Lhuillier's) paper (meaning that certain other forms would be preferable for the house); and he is right. In fact Lambert has not at all considered the general problem, what would be the best form for the hexagonal house? but merely the particular case which arises in connexion with the bee-cell, viz., if the base and capacity are given and the roof is to be rhomboidal, what must the angles of the rhombs be?

The next discussion of the form of the bee's cell that I have to notice is by Lord Brougham; and it occurs in vol. i. of his 'Dissertations on subjects of Science connected with Natural Theology, being the concluding volumes of the new edition of Paley's Works,' two vols. 1839*. The essay in question occupies pp. 218–368; but it is only with the parts of it that relate to the matters already noticed in this communication that we are here concerned, and it must be understood that it is only to these portions that any remarks that may be made have reference, unless otherwise stated.

In 1858 Lord Brougham extracted the more salient points from his earlier essay and communicated them to the French Academy†. I cannot find that his memoir has yet been printed by the Academy; but an abstract appears on p. 1024 *et seq.* of vol. xlvi. (1858) of the *Comptes Rendus*; and it is published *in extenso* by its author as Tract vi. (pp. 103–121) of 'Tracts, Mathematical and Physical' (London and Glasgow, 1860). All the statements that will be noticed in this paper appear also in the French memoir, although the wording &c. is sometimes modified

* The essay appears in vol. vi. of the collected works of Lord Brougham, by A. and C. Black, Edinburgh; but the *Appendix of Demonstrations*, containing the solutions of the mathematical questions referred to in the text, is left out; and this is done without even an accompanying statement of so important an omission having been made.

† It is not, however, stated in the memoir (at all events as printed in the 'Tracts' . . . . 1860) that the facts contained in it had been previously published.

and the whole contracted. I shall refer to and quote from the original edition of 1839; but wherever the French memoir of 1858 shows any discrepancy it will be pointed out either in the text or a note. As a *résumé* of moderate length was given in the *Comptes Rendus* for 1858, it seemed not unlikely that some one might have anticipated the ensuing remarks; but an examination of the *Comptes Rendus* from 1858 to 1871 showed that only one communication had in this time been made to the Academy on the subject, viz. by the late Mr. Willich (vol. li. p. 633, 1860), who gives a method of constructing a bee-cell which resembles (and is perhaps identical with) that suggested by Leslie Ellis in his Essay.

I may as well mention at once that Lord Brougham's essay shows that he has read carefully Réaumur and Lhuillier, looked at Maraldi and Maclaurin, and not seen Boscovich; he has, however, written with great confidence, and on him falls the responsibility of perpetuating and stamping with a fresh mark of authority the old fiction that had been exposed three quarters of a century before. His works have been so widely read, and will no doubt continue to be so well known in the future, that he has in all probability given a new and long lease of life to the old and silly fable: his statements appear on the face of them to be the results of such careful study of the original authorities, and they are presented with such assurance, that few readers will imagine that they can be otherwise than correct. The story, as given by him, is in its facts the same as it appears in the extract from Mr. Wood's work at the beginning of this paper* (omitting the paragraph about the logarithms); only he does not state that Maclaurin made any measurements; in fact he expressly points out in a note on p. 244 that Dr. Reid, in his Essays†, is wrong in stating that Maclaurin ascertained the angles "by the most exact mensuration the subject would admit," or solved the problem "by a fluxionary calculus."

In reference to Kœnig's blunder of 2′, Lord Brougham says (p. 244), "possibly it is in the logarithms that he has, by neglecting some decimal places, gone wrong;" and on p. 345, "the error into which Mr. Kœnig fell . . . . . . originated most probably in the tables of sines or in the logarithms which he

* In a note in the French memoir a more detailed allusion is made to Maraldi's measures, and the 110° and 70° are regarded as approximate and the 109° 28′ and 70° 32′ as exact: this was Réaumur's view.

† The passage referred to will be found in chap. ii. (Instinct) of Essay III. of Dr. Thomas Reid's 'Essays on the Active Powers of Man' (orig. ed. 1788); but no history of the problem of the bee-cell is there given; in fact the only sentence that has reference to the matter is the one containing the two errors noticed in the text; and it simply states that the bees have solved the *minimum* question correctly.

used*. Now to both of these suggestions, and to the second especially, there are great objections. Kœnig gave a wrong result as the answer to an arithmetical question; and all that can be said is that he somewhere must have made a mistake in his work: if he neglected too many decimal places, that is of course a blunder as much as if he had written a 5 for a 3; and, as far as I can see, there is no reason to assume that he made this error rather than any other. The second suggestion, so far being most probable, is possible, but highly improbable. Given that a wrong numerical result is published and nothing further, it is 50 to 1 that the blunder was in the computations, not in the tables; besides, Kœnig calculated the saving of wax† (which required no tables or numerical computations) wrongly, so that there is *prima facie* evidence that he did not use sufficient care. Lord Brougham was especially afraid of any error in the tables, and therefore obtained two solutions of Kœnig's problem, in one of which the dihedral angle between two of the rhombs was the quæsitum; and he also desired a friend to investigate the question independently. But all this was quite superfluous; if θ be the smaller angle of the rhombs, the analysis readily gives $\cos\theta=\frac{1}{3}=\cdot 3333333$, so that θ can be obtained at once from a table of natural sines; and any one who knows any thing at all about mathematical tables can see at once, by taking a few differences, whether the table contains an error (particularly so large an error as to produce an alteration of $2'$); but even if this is not done, an obvious transformation gives

$$\tan\tfrac{1}{2}\theta=\sqrt{2}=1\cdot 4142136,$$

and $\frac{1}{2}\theta$ (θ being here the larger angle) can be found directly either from a natural or a logarithmic canon at once; or any number of similar transformations can be made. Lord Brougham uses (p. 246) the singular phrase "difference introduced by the logarithmic approximations;" perhaps the last two words are merely a euphemism for "erroneous table," as it is needless to say that 7-figure logarithms generally give results true to a tenth of a second at least, so that an error of two minutes is out of the question.

Whence Mr. Wood or Mr. Mitchell derived the statement that Kœnig was not to blame, but that the error really was in in the logarithms, I am unable to say; but I think it is a fair hypothesis to suggest that Lord Brougham's, or some similar statement to the effect that the error was most likely in the

* This is intensified in the French memoir, "M'étant assuré . . . . que Kœnig était tombé dans l'erreur par les tables de sinus ou des logarithmes &c.," p. 112.

† Lord Brougham does not mention the fact of Kœnig having erroneously calculated the saving.

tables, has been developed into it by persons not very careful about the truth. The story given by Lord Brougham was sufficiently romantic; but the person who added the climax, and gave it point by the sensational allusion to the ship, certainly deserves to have his services remembered.

Maclaurin suggested that Kœnig did not carry his approximations far enough; but I imagine that he either felt a delicacy about saying in so many words that Kœnig had made a blunder, or that he had in his mind (like Boscovich) the probability of his having obtained a complicated equation and solved it by approximation. But Lord Brougham could not have felt this delicacy a century later; and he certainly did not attach Boscovich's meaning to the word *approximations*.

Lord Brougham objects most strongly to the results obtained by Lhuillier; and his arguments are of so remarkable a character that I hope I shall be excused for quoting his exact words, and the more so as the reasoning certainly must have appeared to its author to carry weight, as it is reproduced (although somewhat more briefly) in the French memoir.

He denies that Lhuillier is right in calculating the saving of wax as only $\frac{1}{51}$; he writes (p. 291):—"It is extremely erroneous to represent the saving as only $\frac{1}{51}$ part. . . . The proportion $\frac{1}{51}$ is obtained by comparing the saving upon the base with the whole wax of the cell, including the walls, and supposing the height of the wall to be to the sides as 5 to 1·387. But why is the wax of the wall to be imported into the calculation, with which it has nothing to do? The question is between two forms of the bottom, not of the whole cell. Suppose two kinds of roof for a house were to be compared in order to choose the one that required least timber; though the whole house might be made of wood, we should only compare the expense of the roofs, and leave out the walls that would be common to both plans; otherwise the relative amount of the saving would depend on the height of the house as well as the shape of the roof. This becomes the more evident in the case of the cells from the circumstance of their depth varying in the same comb, and for the same bee, according to many accidental circumstances. . . . [The author then states the great variations that occur in the depth of cells.] The saving therefore is somewhere about a ninth, and not somewhat less than a fifty-first part."

"But there is another consideration which shows still more strikingly the fallacy of the argument derived from taking the whole walls of the cell into calculation. The thickness of the wax is very different in different parts of the cell, being much greater in the base, that is, in the rhomboidal plates, and the

part of the walls adjoining, the six small triangles, which are formed by a line drawn parallel to the base through the points where the rhomboidal plates cut the walls. This is manifest upon inspection; and I have tried it by weighing equal parts in superficial extent, as far as it was possible, of the base and of the sides, and uniformly found the latter sensibly lighter. It did not seem that the proportion was always the same; but I never found the difference less than in the proportion of 3 to 2. The thickness of the walls varies much more than that of the base in different combs. But any considerable difference between the two portions at once destroys the argument of M. L'Huillier. If it is as 3 to 2, then the saving is nearly an eighth upon the thicker part, and consequently about $\frac{1}{35}$ instead of $\frac{1}{51}$ of the whole."

The ratio of one ninth is found by comparing the bottoms of the cells, the bottom of the actual cell being taken as the three rhombs and six triangles, and the bottom of the hypothetical prismatic cell as the hexagonal floor and the portions of the sides below a horizontal plane through highest points of the rhombs (supposing the cell placed with its axis vertical and mouth upwards); and the ratio of $\frac{1}{35}$ follows by considering the ratio of the saving to the whole amount of wax, the bottom (defined as above) being supposed half as thick again as the rest of the cell.

The above quotations contain, I venture to think, as striking instances of bad reasoning as are often met with in writings relating to mathematical subjects. The comparison is between two kinds of cells; the one requires an amount n of wax, the other an amount $n+\frac{1}{50}n$; and the amount saved is $\frac{1}{50}n$, that is to say, if they built flat-bottomed cells, the bees would have to manufacture $\frac{1}{50}$ more wax than in reality they do manufacture. If a house with a V-roof costs £1000, and with a flat roof £1020, the builder who prefers the former saves £20 out of an expenditure of £1000, or $\frac{1}{50}$ of what he spends*; his saving is not a fifth *of his expenditure* because the walls cost £900, and are the same for both. Lord Brougham says, "Why is the wax of the wall to be imported into the calculation, with which it has nothing to do?" and the reply is, "Why is the wax of the wall to be left out of the calculation? it is manufactured by the bees in exactly the same way as is the wax for the rhombs and triangles, and is part of the cell built by the bees." In effect, Lord Brougham replaces "the bees must have walls for their

* Or, to state the same thing differently, if m be the wax required for the hypothetical cell, $m-\frac{1}{51}m$ represents the amount used in the real cell, so that the saving is $\frac{1}{51}$ of the amount that would have been needed for the hypothetical cell; it is because Lhuillier so regarded it that he writes $\frac{1}{51}$ and not $\frac{1}{50}$.

cells" by "the bees do not manufacture the wax for the walls of their cells," and regards the two assertions as identical. The fact that the depth of the cells is variable is exactly analogous to the fact that the angles of the rhombs vary: none of the cells are perfect mathematical figures; but we consider merely the figure which we believe to represent the mean or typical cell.

In reality Lhuillier's calculation of the saving is under the mark; for if, as Lord Brougham so strongly insists in reference to the *minimum minimorum*, the hexagonal plate that closes the mouth of the cell be taken into account also, then the amount of wax in the real cell is 26·370 against 26·847 in the hypothetical cell, and the saving is only $\frac{1}{55}$ of the wax used, or even less if we consider a comb, as each cell has its own hexagonal plate, while the sides and rhombs are, except in the outside cells, common to two contiguous cells.

With regard to the argument drawn from the increased thickness of the rhombs and triangles, it is only necessary to remark that, unless we know the reason for this thickening (which we do not), its recognition removes the question of the bee-cell from the province of mathematics (or exact reasoning) entirely. We are comparing the real cell that the bees do make with a hypothetical cell which, *à priori* might have appeared more simple and suitable; and unless we are sure why the bottom (using the word as above defined) is thicker than the walls, how can we know how much of the hypothetical cell would require to have been thickened if it had been adopted? If the change of thickness destroys Lhuillier's argument, it at the same time destroys all mathematical reasoning as applied to the question. Lord Brougham assumes that what I have called the bottom of the hypothetical cell would have needed thickening had that form been adopted; but this assumption is purely arbitrary, and how much (if any) of the walls would have required to have been thickened is merely matter of opinion; and the question is thereby removed out of the reach of mathematics, where it is not my object to follow it.

I purposely abstain from any remarks on Lhuillier's *minimum minimorum* and Lord Brougham's objections, partly because I may possibly make the problem there treated of the subject of a separate communication at some future time, and partly because so shallow a cell (whether the hexagonal plate be excluded or not) is clearly unsuitable for the other purposes to which the bees apply their cells besides storing honey. It is unnecessary here to do more than allude to Lord Brougham's remarks on Lhuillier and Castillon in reply to the argument marked *thirdly* in his essay, as any one who has read in this paper what Maraldi, Boscovich * &c.

* Leslie Ellis corrects Lord Brougham for representing that Bosco-

did say will see at a glance how much is and how much is not correct. I pass over also one or two minor slips. Of course Lhuillier deserves Lord Brougham's censure for having failed to notice that Maclaurin's and Boscovich's proofs were geometrical.

I have not hesitated to state plainly the value of Lord Brougham's arguments in my opinion, as the frankness with which he has expressed his sentiments with regard to the writings of those with whom he did not agree renders any particular display of courtesy towards his own views unnecessary. Even Leslie Ellis writes, "Either the habit of an advocate's mind—for Lord Brougham may be regarded as counsel for the bees—or his not having read Maraldi's paper must have been the cause of this omission" [viz. of Maraldi's statement that the angles measured were 110° and 70° : this is partly supplied, as has been noticed, in the French memoir]; and I must say that the quiet dignity of the mathematical writings on the subject contrasts most favourably with his violent advocacy. The impression any one would receive from reading Lord Brougham's remarks on Lhuillier and Castillon would be that they had treated the matter ignorantly and unfairly; whereas their memoir seems to me (and I believe it would to any one else) an impartial (though not very brilliant) attempt to add to our knowledge and advance truth. As the result of a tolerably careful examination of the whole question, I may be permitted to say that I agree with Lhuillier in believing that the economy of wax has played a very subordinate part in the determination of the form of the cell; in fact I should not be surprised if it were acknowledged hereafter that the form of the cell had been determined by other considerations, into which the saving of wax did not enter (that is to say, did not enter sensibly; of course I do not mean that the amount of wax required was a matter of absolute indifference to the bees). The fact of all the dihedral angles being 120° is, it is not unlikely, the cause that determined the form of the cell.

Whenever a prediction is subsequently verified by experiment, there is always a strong tendency to believe in the truth of the theory whereby the prediction was obtained, even if the course of subsequent research lends no additional support to it, but rather the reverse. And the equality of the angles of the rhombs and trapeziums is no doubt the solution of so many different

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vich's remarks imputed dishonesty to Maraldi; and although he had not seen them, he feels no doubt that what Boscovich really did say was merely a reproduction of what Maraldi had himself said. This is so, and Ellis was right; but, curiously enough, although Lord Brougham did not know it and had no ground for supposing it, Boscovich does, as noticed *supra*, suggest that perhaps Maraldi was helped by theory.

questions, that the success of Réaumur's problem no more proved the economy theory than did the discovery of conical refraction prove Fresnel's wave theory. No one doubts that the bee-cell is the best that could be contrived for the purposes to which the bees apply it; but the question is, what is the special reason for the three rhombs and their particular form? Lhuillier's *minimum minimorum* is of little use, as he himself points out; for no doubt the shape of the insect &c. require a cell of some depth (and perhaps an apex); but his conclusion that economy is not the primary reason is, I think, in accord with the evidence.

In an interesting memoir in the Cambridge Transactions*, Whewell has drawn special attention to the manner in which hypotheses may become gradually transformed as new facts are discovered, so as sometimes to become almost the opposite of what they were originally, without their adherents acknowledging any defeat. Thus the Cartesians, little by little, modified their vortices by rejecting or altering portions now and then as they were shown to be in opposition to the facts, introducing fresh suppositions, &c., until at length a vortex merely became a complicated piece of machinery for producing a central force, and the Cartesians and Newtonians then ceased to differ, as the vortex, being no longer essential, could be ignored at pleasure—and this without the former ever yielding or formally confessing defeat. Now the problem of the bee-cell seems very like an hypothesis undergoing transformation. Réaumur (though he only speaks of it as the reason, or one of the reasons) and Maclaurin evidently thought saving of wax paramount; Boscovich gave prominence to other reasons also; and Lhuillier believed economy played only a very subordinate part. Lord Brougham vehemently supports the economy hypothesis, but says it is only one reason among several; and he ridicules Lhuillier for thinking that Réaumur &c. asserted it to be the *only* reason. This is the first step in the transformation; and it is quite possible the economy-theorists may in the future, while still contending for their hypothesis, gradually admit that more and more weight is due to the other reasons, till at length they may be contented with an acknowledgment that economy was not ignored—when the transformation will be effected.

I think the bee-cell problem is of sufficient importance, as one of the most remarkable instances of instinct, to merit the space that has been devoted to it. It seems curious that so obvious and easily exposed a story should have been so tenacious of life; but the explanation no doubt is to be sought in the fact that it lies on the borders of two sciences. In a mathematical point of

* "On the Transformation of Hypotheses in the History of Science," vol. ix. part ii.

view the problem is simple, not to say trivial, and possesses but slight interest to a mathematician; and to the naturalist it is too mathematical to be pleasing. A mathematician cannot sometimes help feeling some regret that the great discoveries and advances in his science can be only known to the very few who have learnt to read the language that can alone express them; but from the contents of this communication more than one argument might be drawn to show that it is any thing but an unmixed gain to a science to admit of popularization.

Cambridge, June 10, 1873.

XII. *On the Integration of the Accurate Equation representing the Transmission in one Direction of Sound through Air, deduced on the Ordinary Theory.* By Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge**.

THE following investigation, independently of its bearing on the problem to which it ostensibly relates, I offer as an essay towards a complete theory of the solution of linear partial differential equations of the second order.

The transmission of sound through air in one direction may be represented on the ordinary theory by the single equation

$$0=\frac{d^2\alpha}{dt^2}-\frac{a^2}{\alpha_x^2}\frac{d^2\alpha}{dx^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where α and x respectively denote the ordinate of the particle at the time t and in the state of rest, and where $\alpha_x=\frac{d\alpha}{dx}$—or by the pair of equations,

$$\left.\begin{aligned} 0&=\frac{dv}{dt}-\frac{a^2}{\alpha_x^2}\frac{d\alpha_x}{dx}, \\ 0&=\frac{dv}{dx}-\frac{d\alpha_x}{dt}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where v and α_x are no longer to be regarded as the partial differential coefficients of α, but as independent functions of x and t.

The solution of this last pair of equations must consist of a pair of relations between the variables $v\alpha_x x$ and t, such as

$$F_1(xtv\alpha_x)=0, \quad F_2(xtv\alpha_x)=0; \quad . \quad . \quad . \quad . \quad . \quad (3)$$

from which it is evident that (1) must be derivable from a pair of first integrals of the form indicated by equations (3) when we substitute in them $\frac{d\alpha}{dt}, \frac{d\alpha}{dx}$ for v and α_x respectively.

* Communicated by the Author.

I. Supposing (1) to be derivable from one only of equations (3)*, as, for instance, the first, the form of F_1 can readily be determined. For, differentiating $F_1 = 0$ with respect to t and x separately, we get

$$\left.\begin{aligned} 0 &= F'_1(t) + F'_1(v)\frac{d^2\alpha}{dt^2} + F'_1(\alpha_x)\frac{d^2\alpha}{dx\,dt}, \\ 0 &= F'_1(x) + F'_1(v)\frac{d^2\alpha}{dx\,dt} + F'_1(\alpha_x)\frac{d^2\alpha}{dx^2}, \end{aligned}\right\} \quad . \quad . \quad (4)$$

the elimination of $\frac{d^2\alpha}{dx\,dt}$ between which gives

$$0 = F'_1(v)F'_1(t) - F'_1(\alpha_x)F'_1(x) + \overline{F'_1(v)}|^2\frac{d^2\alpha}{dt^2} - \overline{F'_1(\alpha_x)}|^2\frac{d^2\alpha}{dx^2};$$

and in order that this may coincide with (1), we must have

$$\frac{\overline{F'_1(\alpha_x)}|^2}{F'_1(v)} = \frac{a^2}{\alpha_x^2},$$

$$F'_1(v)F'_1(t) - F'_1(\alpha_x)F'_1(x) = 0. \quad . \quad . \quad . \quad (5)$$

The first of these gives us

$$\frac{F'_1(\alpha_x)}{F'_1(v)} = \pm\frac{a}{\alpha_x},$$

or

$$0 = F'_1(\alpha_x) \mp \frac{a}{\alpha_x}F'_1(v); \quad . \quad . \quad . \quad . \quad (6)$$

the auxiliary equations for the integration of which by Lagrange's method are

$$0 = dv \pm \frac{a}{\alpha_x}d\,.\alpha_x, \quad 0 = dx, \quad 0 = dt;$$

whence we have

$$\begin{aligned} v \pm a\log_\epsilon \alpha_x &= \text{const.}, \\ x &= \text{const.}, \\ t &= \text{const.}; \end{aligned}$$

and therefore

$$\begin{aligned} F_1(txv\alpha_x) &= \phi\left\{(v \pm a\log_\epsilon \alpha_x), x, t\right\} \\ &= \phi\left\{\omega, x, t\right\}, \end{aligned}$$

suppose, where $\omega = v \pm a\log_\epsilon \alpha_x$.

But this value of F_1 must satisfy (5); hence, since

* This is, of course, not a necessary supposition. It will obviously be sufficient if (1) can be derived from any combination of the pair of equations constituting the solution and their derivatives.

$$F'_1(v) = \phi'(\omega),$$

$$F'_1(\alpha_x) = \pm \frac{a}{\alpha_x} \phi'(\omega),$$

$$F'_1(t) = \phi'(t),$$

$$F'_1(x) = \phi'(x),$$

we must have

$$0 = \phi'(\omega) \cdot \left\{ \phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) \right\};$$

i. e. we must have either $\phi'(\omega) = 0$, or $\phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) = 0$.

The first of these would give us

$$F_1(txv\alpha_x) = \phi(x, t),$$

which is obviously an inadmissible result. Adopting, therefore, the alternative hypothesis, or

$$\phi'(t) \mp \frac{a}{\alpha_x} \phi'(x) = 0,$$

and integrating this equation by Lagrange's method, the auxiliary equations for which in this case are

$$0 = dx \pm \frac{a}{\alpha_x} dt, \quad 0 = dv, \quad 0 = d\alpha_x,$$

we shall get

$$\phi = \phi \left\{ \left(x \pm \frac{a}{\alpha_x} t \right), v, \alpha_x \right\}.$$

But we have already seen that ϕ is of the form shown by the equation

$$\phi = \phi \{ \omega, x, t \};$$

and these forms of ϕ are incompatible except on the supposition that $\phi'(t) = \phi'(x) = 0$—in other words, unless we have

$$F_1(txv\alpha_x) = \phi(\omega).$$

Therefore the first of equations (3) becomes

$$\phi(\omega) = 0,$$

or, which amounts to the same thing,

$$\omega = 0,$$

or

$$v \pm a \log_\epsilon \alpha_x = 0. \qquad \text{(7)}$$

When (7) holds, substituting in (1) we get

$$0 = \frac{d\alpha_x}{dt} \pm \frac{a}{\alpha_x} \frac{d\alpha_x}{dx},$$

which, integrated by Lagrange's method, gives

$$\alpha_x=\psi\left\{x\mp\frac{a}{\alpha_x}t\right\}. \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8)$$

Equations (7) and (8) together constitute a solution of (1) which, making allowance for the difference of the independent variables employed, is identical with that given by Poisson in the *Journal de l'École Polytechnique* for 1807*.

II. The foregoing result, it will be remembered, has been derived on the assumption that (1) is derivable from a single one of the equations (3) and its derivatives. I now propose to investigate the forms which F_1 and F_2 must assume in order that (1) may be derivable from a combination of equations (3) and their derivatives.

The differentiation of $F_2=0$ gives us

$$\left.\begin{aligned} 0&=F'_2(t)+F'_2(v)\frac{d^2\alpha}{dt^2}+F'_2(\alpha_x)\frac{d^2\alpha}{dx\,dt},\\ 0&=F'_2(x)+F'_2(v)\frac{d^2\alpha}{dx\,dt}+F'_2(\alpha_x)\frac{d^2\alpha}{da^2}. \end{aligned}\right\} \quad . \quad . \qquad (9)$$

* When the motions are small the result is altogether different; for in that case (6) becomes

$$0=F'_1(\alpha_x)\mp aF'_1(v),$$

which, the auxiliaries for its integration being

$$0=dv\pm a\,d\alpha_x,\quad 0=dx,\quad 0=dt,$$

gives us

$$F_1=\phi\{\overline{\omega},\ x,\ t\},$$

where $\overline{\omega}=v\pm a\alpha_x$. Hence we have

$$F_1'(v)=\phi'(\overline{\omega}),\qquad F_1'(t)=\phi'(t),$$

$$F_1'(\alpha_x)=\pm a\phi'(\overline{\omega}),\quad F_1'(x)=\phi'(x);$$

and substituting these values, (5) becomes

$$0=\phi'(t)\mp a\phi'(x).$$

The auxiliaries for the integration of this last are

$$0=dx\pm adt,\quad 0=d\overline{\omega},$$

which give us

$$\phi=\phi\{(v\pm a\alpha_x),\ (x\pm at)\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (8a)$$

instead of, as in the case of the accurate equation treated of in the text,

$$\phi=\phi(v\pm a\log_\epsilon \alpha_x).$$

Hence, when the motions are small, we may assign to F_1, F_2 the alternative values of ϕ given by (8*a*); or, which is the same thing, we may take for our solution

$$v+a\alpha_x=\psi_1(x+at),$$

$$v-a\alpha_x=\psi_2(x-at),$$

which is identical with the ordinary solution given in this case. We cannot, however, adopt the same course in the general case when the motions are not small.

Eliminating $\frac{d^2\alpha}{dx\,dt}$ between the first of equations (4) and the last of (9), and between the last of (4) and the first of (9), we get

$$0=F'_2(v)F'_1(t)-F'_2(x)F'_1(\alpha_x)+F'_2(v)F'_1(u)\frac{d^2\alpha}{dt^2}-F'_2(\alpha_x)F'_1(\alpha_x)\frac{d^2\alpha}{dx^2},$$

$$0=F'_2(\alpha_x)F'_1(x)-F'_2(t)F'_1(v)+F'_2(\alpha_x)F'_1(\alpha_x)\frac{d^2\alpha}{dx^2}-F'_2(v)F'_1(u)\frac{d^2\alpha}{dt^2},$$

the addition of which equations gives

$$0=F'_2(\alpha_x)F'_1(x)-F'_2(t)F'_1(v)+F'_2(v)F'_1(t)-F'_2(x)F'_1(\alpha_x). \quad (10).$$

Again, eliminating $\frac{d^2\alpha}{dx\,dt}$ between the first of each of the pairs (4) and (9), and also between the last of each of the same pairs, we get

$$0=F'_2(\alpha_x)F'_1(t)-F'_2(t)F'_1(\alpha_x)+\{F'_2(\alpha_x)F'_1(v)-F'_2(v)F'_1(\alpha_x)\}\frac{d^2\alpha}{dt^2},$$

$$0=F'_2(v)F'_1(x)-F'_2(x)F'_1(v)+\{F'_2(v)F'_1(\alpha_x)-F'_2(\alpha_x)F'_1(v).\}\frac{d^2\alpha}{dx^2}.$$

Hence, if (1) holds, we shall have, eliminating $\frac{d^2\alpha}{dt^2}, \frac{d^2\alpha}{dx^2}$ between the last two equations and (1),

$$0=F'_2(\alpha_x)F'_1(t)-F'_2(t)F'_1(\alpha_x)+\frac{a^2}{\alpha_x^2}\{F'_2(u)F'_1(x)-F'_2(x)F'_1(u)\}. \quad (11)$$

Equations (10) and (11) express the conditions to be satisfied by F_1, F_2 in order that (1) may be derivable from a combination of the derivatives of (3). Transform these equations by putting

$$F_1(xtv\alpha_x)=f_1(xt\omega_1\omega_2),$$
$$F_2(xtv\alpha_x)=f_2(xt\omega_1\omega_2),$$

where

$$\left.\begin{array}{l}\omega_1=v+a\log_\epsilon\alpha_x,\\ \omega_2=v-a\log_\epsilon\alpha_x,\end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

then we shall have

$$F_1'(v)=f_1'(\omega_1)+f_1'(\omega_2), \qquad F_2'(v)=f_2'(\omega_1)+f_2'(\omega_2),$$

$$F_1'(\alpha_x)=\frac{a}{\alpha_x}\{f_1'(\omega_1)-f_1'(\omega_2)\}, \qquad F_2'(\alpha_x)=\frac{a}{\alpha_x}\{f_2'(\omega_1)-f_2'(\omega_2)\},$$

$$F_1'(t)=f_1'(t), \qquad F_2'(t)=f_2'(t),$$

$$F_1'(x)=f_1'(x). \qquad F_2'(x)=f_2'(x).$$

Substituting these values in (10) and (11), they become

$$0=\frac{a}{\alpha_x}f_1'(x)\{f_2'(\omega_1)-f_2'(\omega_2)\}+f_1'(t)\{f_2'(\omega_1)+f_2'(\omega_2')\},$$

$$-f_2'(t)\{f_1'(\omega_1)+f_1'(\omega_2)\}-\frac{a}{\alpha_x}f_2(x)\{f_1'(\omega_1)-f_1'(\omega_2)\},$$

$$0=\frac{a}{\alpha_x}f_1'(t_1)\{f_2'(\omega_1)-f_2'(\omega_2)\}+\frac{a^2}{\alpha_x^2}f_1'(x)\{f_2'(\omega_1)+f_2'(\omega_2)\}$$

$$-\frac{a}{\alpha_x}f_2'(t)\{f_1'(\omega_1)-f_1'(\omega_2)\}-\frac{a^2}{\alpha_x^2}f_2'(x)\{f_1'(\omega_1)+f_1'(\omega_2)\},$$

or

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)$$

$$-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1)+\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2),$$

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)+\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)$$

$$-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1)-\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2).$$

The addition and subtraction of the last two equations give us

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\left\{\frac{a}{\alpha_x}f_2'(x)+f_2'(t)\right\}f_1'(\omega_1),$$

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)-\left\{\frac{a}{\alpha_x}f_2'(x)-f_2'(t)\right\}f_1'(\omega_2);\quad(13)$$

he first of which, when arranged according to the partial differential coefficients of f_2, will stand

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}f_2'(\omega_1)-\frac{a}{\alpha_x}f_1'(\omega_1).f_2'(x)-f_1'(\omega_1)f_2'(t);\quad(14)$$

the auxiliary equations for the integration of which by Lagrange's method are

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)+f_1'(t)\right\}dx+\frac{a}{\alpha_x}f_1'(\omega_1)d\omega_1,$$

$$0=dx-\frac{a}{\alpha_x}dt,$$

$$0=d\omega_2.$$

Of these equations, the first by means of the second may be put under the form

$$0=f_1'(x)dx+f_1'(t)dt+f_1'(\omega_1)d\omega_1,$$

or, since $d\omega_2=0$,

$$0=f_1'(x)dx+f_1'(t)dt+f_1'(\omega_1)d\omega_1+f_1'(\omega_2)d\omega_2$$
$$=d.f_1.$$

Hence we may take for the auxiliary equations,

$$\left.\begin{aligned} 0&=df_1, \\ 0&=dx-a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.dt, \\ 0&=d\omega_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

(since by (12) we have $\log_\epsilon \alpha_x = \frac{\omega_1-\omega_2}{2a}$), the integrals of which are

$$f_1=C,$$
$$\int\mu\{dx-a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.dt\}=C'^*,$$
$$\omega_2=C''.$$

Hence, putting m for the equivalent of C′, we get for the value of f_2 derived from the integration of (14),

$$f_2=f_2\{f_1, m, \omega_2\},$$

or, since $f_1=0$,

$$f_2=f_2\{m, \omega_2\}.$$

On the other hand, equation (13) may be written

$$0=\left\{\frac{a}{\alpha_x}f_1'(x)-f_1'(t)\right\}f_2'(\omega_2)-\frac{a}{\alpha_x}f_1'(\omega_2)f_2'(x)+f_1'(\omega_2)f_2'(t). \quad (16)$$

1. Hence, if this last be a substantive equation between the partial differential coefficients of f_2, we shall have for its integration, following a method precisely similar to that already adopted, the auxiliary equations

$$0=df_1,$$
$$0=dx+a\epsilon^{\frac{\omega_2-\omega_1}{2a}}dt,$$
$$0=d\omega_1;$$

and, as before, we shall arrive at a solution of the form

$$f_2=f_2\{n, \omega_1\}.$$

It is clear, however, that if we have

$$f_2=\text{funct.}\ (m, \omega_2)$$

and also

$$f_2=\text{funct.}\ (n, \omega_1),$$

we must have

$$m=\text{funct.}\ (n, \omega_1)$$

and

$$\omega_2=\text{funct.}\ (n, \omega_1).$$

* Note that in the integration of (15) we shall have ω_2 constant; so that by means of the equation $f_1=C$ we may express ω_1 in terms of x and t and of quantities which are to be regarded as constant. Thus (15) will always be integrable by means of a factor μ.

Hence, eliminating n between these last, we shall get

$$m = \text{funct.}\ (\omega_1\omega_2),$$
$$\therefore f_2 = f_2(m\omega_2) = \text{funct.}\ (\omega_1\omega_2);$$

therefore one of the first integrals of (1) will be of the form

$$\text{funct}\ (\omega_1\omega_2) = 0,$$

whence it is clear that we must have

$$v = f(\alpha_x).$$

The substitution of this value of v in equations (2) gives us

$$0 = f'(\alpha_x)\frac{d\alpha_x}{dt} - \frac{a^2}{\alpha_x^2}\frac{d\alpha_x}{dx},$$

$$0 = f'(\alpha_x)\frac{d\alpha_x}{dx} - \frac{d\alpha_x}{dt};$$

from which it follows that we must have

$$\overline{f'(\alpha_x)}|^2 = \frac{a^2}{\alpha_x^2},$$

and

$$f'(\alpha_x) = \pm\frac{a}{\alpha x};$$

$$\therefore\ v = \pm a \log_\epsilon \alpha x + \mathrm{C},$$

or, since v vanishes when $\alpha_x = 1$,

$$v = \pm a \log_\epsilon \alpha\ ;$$

from which it is clear that the solution obtainable in this manner must be identical with that already deduced.

2. The only way of escape from this conclusion is to suppose that (16) is illusory, considered as a relation between the partial differential coefficients of f_2—or, in other words, that we have

$$\frac{a}{\alpha_x} f_1'(\alpha_x) - f_1'(t) = 0,$$
$$f_1'(\omega_2) = 0.$$

The last of these implies that f_1 is a function of ω_1, x, and t only; hence, writing the former

$$a\epsilon^{-\frac{\omega_2-\omega_1}{2a}} f_1'(x) - f_1'(t) = 0,$$

the auxiliary equations for its integration are

$$0 = a\epsilon^{-\frac{\omega_2-\omega_1}{2a}} dt + dx, \quad 0 = d\omega_1,$$

which give us

$$f_1 = f_1\{\omega_1, (a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t+x)\}$$

$$= f_1\{\omega_1, k\}$$

suppose, where $k = a\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t+x.$

But

$$f_1'(\omega_2) = \frac{1}{2a}.\epsilon^{\frac{\omega_2-\omega_1}{2a}}.t.f_1'(k);$$

therefore, since $f_1'(\omega_2)=0$, we must have $f_1'(k)=0$, and therefore

$$f_1 = f_1(\omega_1);$$

that is, one of the equations of our solution must be of the form

$$f_1(\omega_1)=0,$$

a conclusion the consequences of which have been already pointed out.

On the whole, therefore, it appears that the pair of equations

$$v = \pm a \log_\epsilon \alpha_x,$$

$$\alpha_x = \psi\left\{x \mp \frac{a}{\alpha_x}t\right\}$$

constitute the most general solution of which the equations (2) and (1) are susceptible when the motions are not small. But $\alpha_x = \frac{\mathrm{D}}{\rho}$, where D denotes the density of equilibrium. Hence, as it is clearly impossible that we shall *always* have the relation between the velocity and density indicated by the equation

$$\frac{v}{a} = \pm \log_\epsilon \frac{\rho}{\mathrm{D}}$$

when the motions are not small, it follows that the equations (2) and (1) must be considered defective in their representation of the motion.

6 New Square, Lincoln's Inn,
March 1873.

XIII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* By James D. Dana.

[Continued from p. 54.]

5. *Mountain-making slow work.*

TO obtain an adequate idea of the way in which lateral pressure has worked, it is necessary to remember that mountain-elevation has taken place after immensely long periods of quiet and gentle oscillations. After the beginning of the Primordial, the first period of disturbance in North America of special note was that at the close of the Lower Silurian, in which the Green Mountains were finished; and if time from the beginning of the Silurian to the present included only fifty millions of years—which most geologists of the present day would consider much too small an estimate—the interval between the beginning of the Primordial and the uplifts and metamorphism of the Green Mountains was at least ten millions of years. The next epoch of great disturbance in the same Appalachian region was that at the close of the Carboniferous era, in which the Alleghanies were folded up—by the above estimate of the length of time, thirty-five millions of years * after the commencement of the Silurian; so that the Appalachians were at least thirty-five millions of years in making, the preparatory subsidence having begun as early as the beginning of the Silurian. The next on the Atlantic border was that of the displacements of the Connecticut-river sandstone, and the accompanying igneous ejections, which occurred before the Cretaceous era—at least seven millions of years, on the above estimate of the length of time, after the Appalachian revolution. Thus the lateral pressure resulting from the earth's contraction required an exceedingly long era in order to accumulate force sufficiently to produce a general yielding and plication or displacement of the beds, and start off a new range of prominent elevations over the earth's crust.

6. *System in the mountain-making movements on the opposite borders of the North American Continent and over the Oceanic areas.*

A summary of the general system of movements and mountain-making on the opposite borders of the continent, and over the oceanic areas, will, I think, render it apparent that the views here sustained have a broad foundation.

I omit any special reference to the Archæan elevations, and

* These estimates of the relative lengths of ages are based on the maximum thickness of their rocks—very uncertain data, but the best we have.

also the local disturbances in the Primordial of Newfoundland, as well as the facts relating to minor changes of level.

a. Mountain-making on the Atlantic border.

1. At the close of the Lower Silurian, or a little earlier, a culmination of the great Appalachian geosynclinal resulted in displacements, plications, and metamorphism, and the making of a *synclinorium* along the *Green-Mountain* region, these mountains (some summits at present over 4000 feet high above the sea) being the result. The depth to which the region subsided during the Lower Silurian era, and the thickness of the accumulations, are not ascertained; probably the extent was not less than 20,000 feet.

2. Simultaneously a *permanent anticlinorium* was made over the Cincinnati region, from Lake Erie into Tennessee, parallel to the Alleghanies of Virginia, 250 miles to the north-west.

3. The Acadian region (embracing western Newfoundland, St.-Lawrence Bay, the Bay of Fundy, and part of Nova Scotia and New Brunswick adjoining, and probably the sea south-west between St. George's Bank and the coast of Maine, with also an area in Rhode Island) was the course of a great geosynclinal, or a series of them, parallel in general direction to that of the Appalachian region; it continued in progress, but with mountain-making interruptions and some shift of position to the eastward, from the Silurian to the close of the Jurassic.

At the close of the Lower Silurian no general disturbances occurred in this Acadian region, so far as is known. In the Anticosti seas, or northern part of St. Lawrence Bay, limestones, as Logan states, were uninterruptedly in formation from the beginning of the Hudson period of the Lower Silurian to the middle of the Upper Silurian, showing that the Acadian geosynclinal was then in regular progress. It so continued until the close of the Devonian, when disturbances, plication, and metamorphism took place in eastern Canada, Nova Scotia, and the bordering region of New Brunswick, and the most extensive of Acadian Palæozoic synclinoria resulted, according to the observations of Dr. Dawson and others.

4. The close of the Carboniferous age was an epoch of mountain-making in the Alleghany region—the Alleghanies from New York to Alabama having been then made, as already explained.

5. At the same time there were disturbances and synclinorian plications in the Acadian region. During the Carboniferous era, according to Logan and Dawson, 16,000 feet of rock had in some parts accumulated; and therefore a geosynclinal of 16,000 feet formed—the rocks in their many coal-beds and root-

bearing layers bearing evidence, to the last, of oscillations involving an intermittent but progressing subsidence. The synclinorium, the resultant, was much less marked than that at the close of the Devonian.

6. During the Palæozoic, along the sea-border, a more or less perfect barrier was made by a geanticlinal uplift (anticlinorium), which was a counterpart to the geosynclinal of the Appalachian region*.

7. The middle or close of the Jurassic period was an epoch of displacements, and the making of a series of imperfect synclinoria along the Triassico-Jurassic areas from Nova Scotia to southern North Carolina, as sufficiently described.

8. During the era of the Connecticut-river sandstone (Triassico-Jurassic) a nearly complete sea-border anticlinorium existed, a counterpart to the progressing geosynclinal. Its existence is proved by the absence of all marine fossils from the beds*.

9. The era closing the Cretaceous, and that of the Tertiary, witnessed but small uplifting and some local displacements of the rocks of these eras on the Atlantic border. The principal movement was geanticlinal; and it involved probably the whole Alleghany region.

10. In the Quaternary there were extended movements of geanticlinal and geosynclinal character, which need not be here described.

b. Mountain-making after Archæan time on the Pacific border, within the territory of the United States.

1. At the close of the Lower Silurian, none yet known.
2. At the close of the Devonian, none yet known†.
3. At the close of the Carboniferous age, or the Palæozoic,

* In my 'Manual of Geology' the probable existence of such a barrier is recognized, in connexion with the remarks on the geography of the Trenton period in America; and it is particularly dwelt upon and illustrated by a map in the chapter on the Triassic; but it is not spoken of as connected in origin with the geosynclinal that was in progress to the west of it. Evidence with regard to this anticlinorium is given in the following part of this memoir.

† Although no case of unconformability between the Carboniferous and the underlying Palæozoic is yet distinctly made out in the Sierra Nevada, the Great Basin, or the Wahsatch, such occur further south, according to Mr. J. W. Powell, in the vicinity of the Grand Cañon of the Colorado (see Amer. Journ. Sci. 3rd ser. vol. v. p. 457). The fact that Whitney has found no rocks lower than Carboniferous in the Sierra may be a consequence of the same unconformability beneath these mountains. But in the region of the Cañon, the Carboniferous, Triassic, Jurassic, Cretaceous, and Tertiary beds are all conformable. The epochs of mountain-making over the Pacific slope south of the latitudes of the Wahsatch range, and also of that to the north, were different from those within these latitudes.

none yet known; and if none really occurred, then the contracting globe at that time, as far as U. S. North America is concerned, must have expended its energies (which it had been gathering during the Palæozoic) in making the Alleghanies, and in some minor plications along the Acadian region.

The "Great Basin," between the Sierra Nevada on its western border and the Wahsatch range on its eastern (lying along the meridian just east of the Great Salt Lake), contains a number of short ridges parallel to these lofty border ranges, some of which are quite high*; and they consist, according to King, "of folds of the infra-Jurassic rocks;" and "it is common to find no rocks higher than the Carboniferous," owing, it is stated, to the erosion that has taken place. It is not clear that part, at least, of the Great Basin plications may not have taken place before the Jurassic era. If not, then the movements must have been in some way involved with those of the Sierra and Wahsatch regions†.

4. At the close of the Jurassic two great geosynclinals, which had been in progress through the Palæozoic and until this epoch in the Mesozoic, culminated each in the making of a lofty synclinorium:—one the Sierra Nevada, some of whose summits are over 14,000 feet high; the other the high Wahsatch, a parallel north and south range.

Whitney has proved that the Carboniferous and Jurassic rocks are comformable in the Sierra-Nevada range, and that the close of the Jurassic was the epoch of its origin; but direct proof is not yet found that the Devonian and Silurian forma-

* An admirable chart, giving in detail the topography of this whole region and including the Wahsatch, has been prepared by Mr. James T. Gardner, after careful surveys by himself, topographical surveyor of the Exploration of the Fortieth Parallel under Clarence King, and is now ready for the engraver. Mr. King has published thus far only brief chapters on the geological results of his survey in the volume of J. T. Hague on Mining Industry, vol. iii. He has ready for publication vols. i. and ii., on Systematic and Descriptive Geology. The Botanical Report of the Survey, vol. v., has been issued; but vol. iv., on Zoology and Palæontology, remains to be completed.

† Mr. James T. Gardner, in a letter of May 8th, informs me that in his opinion all the more important mountain-ranges of the Great Basin (in Nevada) are included in a chain trending about N. 40° E., the whole having a breadth across the trend of 120 miles. Austin, in Nevada, lies near the centre line of the chain. To the west of this elevated region is the great depression where the rivers of Nevada evaporate in Carson, Humboldt, Pyramid, Mud, and other lakes; and to the east is the great depressed area of Salt Lake. On this view, if these mountains were made at the close of the Jurassic, there were formed at this epoch three lofty synclinoria (the Sierra, the Humboldt, and the Wahsatch) on the western-border, central, and eastern-border chains of the Great Basin. The precise determination of the epoch of origin of the Humboldt chain is therefore of much importance.

tions are included. The granite axis of the chain probably indicates, as LeConte has suggested, the region of maximum disturbance and metamorphism.

The Wahsatch contains, according to Clarence King, formations of all the ages from the Lower Silurian to the Jurassic; and the whole are throughout conformable; and a great thickness of crystalline rocks exists beneath, supposed to be Archæan, which he states are conformable also. The plications and mountain-making took place, as King states, contemporaneously with the same in the case of the Sierra, before the Cretaceous era, the Cretaceous beds lying on the Jurassic unconformably.

These two synclinoria are 400 miles apart. The preparatory geosynclinal of the Wahsatch, and probably that of the Sierra, took for its completion, supposing it to have begun with the opening Silurian, a period at least a fifth longer than the whole Palæozoic.

5. At the close of the Cretaceous another pair of geosynclinals, parallel with the coast, but geosynclinals of only Cretaceous origin, culminated in synclinoria.

One of the Cretaceous geosynclinals was in progress *east of the Wahsatch,* along the whole summit-region of the Rocky Mountains in the United States. Directly east of the Wahsatch, according to King, the beds are 9000 feet thick, or more; and, as Hayden states, they have a great thickness in the Laramie Plains, and little less over the upper Missouri region, so that the downward movement was in some parts a profound one, and affected a very wide extent of country. Hayden and King make this disturbance to have taken place after part, or all, of the Eocene period had passed, while Prof. Marsh holds that it occurred at the close of the Cretaceous period*.

* Clarence King has very briefly described the Wahsatch region, as well as the country to the west, in the third volume (4to, 1870) of his United-States Geological Exploration of the 40th parallel; and on page 454 he says:—"Subsequent to the laying down of the old cretaceous system, and of those conformable freshwater beds which close the coal-bearing period, another era of mountain-uplifts occurred, folding the coal series [Cretaceous and Lower Tertiary] into broad undulating ridges having a general trend north-east." He then observes that freshwater Tertiary beds of sand and clay, "an immense accumulation," were laid down *unconformably* over this upturned Cretaceous, and, after the Miocene era, were subjected to "orographic" disturbances and "tilted to an angle of 15° to 20°, or thrown into broad and gentle undulations wherever they lie in the neighbourhood of the older ranges, such as the Wahsatch and Uintah." These disturbances were confined to within fifteen miles of the Wahsatch. The period in which they occurred witnessed also great outflows of trachytic rocks in this and other parts of the Rocky-Mountain region. Mr. King adds, on page 455, that there is no question as to the identity of the beds that *overlie unconformably* the Cretaceous folds along the eastern flank of the Wahsatch with the horizontal Tertiary deposits of the Green-River

The other geosynclinal belt of the Cretaceous era was to the *west of the Sierra Nevada,* as described by Whitney. This coast geosynclinal ended in extensive displacements and plications, much metamorphism, and a high synclinorium.

6. The intermediate region, the Great Basin, which had been widened at the close of the Jurassic by the annexation of the plicated and consolidated Sierra and Wahsatch, was the era of a geanticlinal, or at least of absence of subsidence; for King says no Cretaceous rocks occur over it.

7. With the close of the Cretaceous, or when the Cretaceous synclinorian movements of the sea-coast and mountains were ending, a geanticlinal movement of the whole Rocky-Mountain region began, which put it above the sea-level, where it has since remained. This upward movement continued through the Tertiary.

8. During the Tertiary age, until the close, probably of the Miocene Tertiary, another pair of parallel geosynclinals, but geosynclinals of Tertiary formation, were in progress. The Cretaceous synclinoria had given still greater breadth and stability to the relatively stable region between them; and one of these new troughs is hence further east on the mountain side, and the other further west on the coast side.

In the coast geosynclinal, marine tertiary beds were accumulated to a thickness of 4000 to 5000 feet; and then followed the epoch of disturbance ending in another coast synclinorium, a coast range of mountains, in some places metamorphic, and having ridges, many of which are at present 2000 feet or more in height above the sea, and some in the Santa-Cruz range, according to Whitney, over 3500 feet.

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basin, and that over this basin between the Green River and the Wahsatch no single instance of conformity occurs between the coal-beds and the overlying horizontal freshwater strata. As stated above, he makes the epoch of Cretaceous uplifting to have followed, not the Cretaceous period, but the earliest period of the Tertiary, Eocene beds being, in his view, included with the Cretaceous in the folds referred to.

Dr. Hayden has investigated with much detail the Green-River basin and the region east of it, and years since announced that the Lower Tertiary beds in some parts of the Rocky-Mountain region were tilted at a high angle. He has held that all the coal-bearing strata were Lower Tertiary, but now agrees with the view expressed by King, and first suggested by Meek, that part are Cretaceous, while another part are Lower Tertiary, and considers the later Tertiary beds (which lie unconformably on the beds below in the regions of disturbance) Miocene and Pliocene. He states that the thickness of the Cretaceous formation in the Laramie plains is 8000 to 10,000 feet. He observes in a recent letter to the writer that the coal-bearing strata and Cretaceous are never unconformable, but instead are often folded together, and sometimes stand at a high angle, even vertical in many places—as in the Laramie Plains south of Fort Sanders, along the

The other is to the east of the Cretaceous axis in the summit region of the Rocky-Mountain chain. A great thickness of freshwater beds was made in the Green-River region and some other places about the Rocky-Mountain summits, and thinner deposits to the eastward. The thickness, in connexion with evidences of shallow-water origin, indicates a progressing geosynclinal, although the ocean gained no entrance to it. The downward bending ended probably just after the Miocene period, without general displacements; but there were tiltings along the more western border of the Tertiary in the vicinity of the Wahsatch and other mountains. (See note on page 135.)

9. Since the Miocene era, and on through much of the Quaternary, there have been vast fissure-eruptions over the western Rocky-Mountain slopes. They had great extent, especially in the Snake-River region, where the successive outflows made a stratum 700 to 1000 feet thick, over an area 300 miles in breadth. There are other similar regions, but of less area.

It is thus seen that along the Pacific side of the continent the crust, under the action of lateral pressure, first bent downward profoundly, and then yielded and suffered fracture and plications, directly along a belt, parallel with the coast, either side of the Great Basin (and perhaps over this basin to some extent), the two great lines 400 miles apart. The plicated regions thus made, having become firm by the continued pressure and the engendered heat and resultant solidification, the crust next bent, and then yielded, in a similar way, along an axis outside of the former regions of disturbance, the two axes over 600 miles apart; and again all was mended in the same way. Then it bent a third time, just outside of the last range, on each side of the same great area, the lines over 700 miles apart; and then over the western of the two ranges the beds were displaced,

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Big-Horn region between Long's Peak and Pike's Peak, near Denver in Colorado, &c. Near the mouth of the Big Horn, the Chetish or Wolf Mountains, consist of these upturned strata, and have a height of 1500 to 2000 feet above the Yellowstone. He found the later Tertiary beds sometimes tilted at a small angle, never over 10°.

The discovery of Dinosaurian remains in some of the coal-beds, announced by Marsh and Cope, and of *Inocerami*, as ascertained by Meek, is one part of the evidence on which the lower parts of the coal-beds are determined to be Cretaceous. Besides this, there is the fact that the supposed Miocene of the Green-River basin contains remains of mammals that are decidedly Eocene in character; and if these are Eocene, then the coal-beds are something older. Professor Marsh is very strongly of the opinion that all the *coal-beds are Cretaceous*.

On the other side, Lesquereux states that the evidence from fossil plants is totally opposed to making any of the coal-strata Cretaceous.

The method of mountain-making, and the principle involved, are the same whatever be the decision as to the exact epoch of the Cretaceous plication.

solidified, and left in high ridges, but over the eastern the final disturbances were local and slight.

There were hence two parallel series, contemporaneous in steps of progress, situated on opposite borders of the Great Basin, a *coast* series and a *mountain* series, each having its highest member toward the basin—the coast series the grandest in its three parts and leaving evidences of the profoundest disturbance and the greatest amount of metamorphism. The Wahsatch range is nearly as high as the Sierra; but probably a fourth of its height is due to the final elevation of the Rocky-Mountain region.

The last bendings were more local than the preceding, because the crust had become stiffened by its plicated and solidified and partly crystallized coatings, as well as by thickening beneath; and, therefore, while the Tertiary movements were in progress the part of the force not expended in producing them carried forward an upward bend, or geanticlinal, of the vast Rocky-Mountain region as a whole. For the same reason profound *breakings* took place where bending was not possible, and thereby immense floods of liquid rock were poured out over the surface. (Most of the great mountains of the globe were lifted about this time—that is, in the course of the Tertiary era—and many of the great volcanoes were made.)

There were irregularities, or exceptional courses, in connexion with this system of movements and their effects. But these show only that in the same area the lateral pressure at work was not alike either in amount or in direction in different latitudes, nor was the resistance before it the same.

The results correspond with the well-understood effects of lateral pressure. Suppose a long beam, having an even texture, except that a portion toward the middle (say a sixth of the whole length) is stouter than the rest, to be subjected at its extremities to direct pressure. The first yielding and fracture would take place toward the stouter portion on either side. If this break were mended by splicing and cementing until firmer than before, the next region of yielding would be just outside of the former. In brief, the fracturing would be in each case near the stouter portion of the beam. Moreover the extent of the yielding and fracture on each side would have some relation to the amount of pressure against that side. Just so has it been with the earth's crust under the action of lateral pressure. The facts further illustrate the truth, before announced, that the force from the ocean side had in some way the advantage, and in fact was the greater. But the full difference is not indicated by the difference in the results of disturbance, since the shoving force on the side of greatest pressure would not be

limited in its action to its own side, unless the intermediate stouter region were wholly immovable.

c. Movements over Oceanic areas.

The history of the changes of level over the oceanic areas is necessarily a meagre branch of geological science. There are, however, some great truths to be gathered which are of profounder import than is generally acknowledged. They show that the oceanic crust has sometimes acted in the capacity of a single area of depression, although of so immense extent. I allude briefly here to only two of the facts, referring the reader to my former articles for a fuller discussion of the subject:—

First, the remarkable one that nearly all the ranges of islands over the Pacific ocean, and even the longer diameters of the particular islands, lie nearly parallel with the great mountain-ranges of the Pacific coast of North America. There is a dynamical announcement in this arrangement—which is partly recognized, when we refer it, as I have proposed, to the existence of directions of easiest fracture in the very nature of the infra-Archæan crust, and regard the courses of these feature-lines of the oceans and continents as having reference to one of these directions. But, besides this, there is a declaration with regard to the *direction* of the pressure that acted against the continents, and reacted over the oceanic areas.

The other fact is that of the coral-island subsidence, already referred to, which affected the tropical ocean for its whole breadth, or more than a quarter of the circumference of the globe—sinking the sea-bottom at least 3000 feet over a large part of the area, and probably over its axial portions two or three times this amount*. The oceanic basin was evidently one basin in its movement; but the areas of less and greater subsidence, of parallel N.W. by W. trend, so alternate along the southern border of the region of subsidence that we may conclude there were great parallel waves, made by lateral pressure in the crust, as I have elsewhere explained†—that is, geosynclinals and geanticlinals, such as are the only possible conditions of the crust under the lateral pressure of the contraction. Now this great oceanic subsidence, involving the breadth of the ocean, if begun in the Tertiary era, as is probable, was going forward at the very time when the Rocky Mountains, and other great mountains of the globe, were in progress of elevation, as if these were counterpart movements in the earth's surface; and it continued

* Author's Rep. Geol. Wilkes U.S. Expl. Exped. 4to, 1849, p. 399; and 'Corals and Coral Islands,' 8vo, 1872, p. 329.

† Rep. Geol. Expl. Exped. p. 399; 'Corals and Coral Islands,' p. 328.

on during the Glacial era, when the continental elevations appear to have reached their highest limit.

We gather from these facts how it is that a general submergence, or an emergence, might characterize cotemporaneously large areas of North America and Europe—as, for example, in the Subcarboniferous, Carboniferous, and Permian periods, during which the rocks show that there was a general parallelism in the movements. If a geanticlinal were in progress over the middle of the Atlantic crust as a result of the lateral thrust in the continental and oceanic crusts there might also be a reverse movement or general sinking along the continental borders, as well as a rise of water about the continents from the diminution in the ocean's depth; and when the oceanic geanticlinal flattened out again through subsidence, the subsiding crust would naturally produce a reverse movement along one or both continental borders.

From the various considerations here presented, derived from both the continental and oceanic areas, it is apparent that the earth has exhibited its oneness of individuality in nothing more fundamentally and completely than in the heavings of its contracting crust.

The subjects of metamorphism, the earth's interior, igneous eruptions and volcanoes remain for discussion. In addition I propose to consider the steps in the origination of the continental plateaux and oceanic basins, and also to present some facts bearing on the general nature of the infra-Archæan crust—that is, the part below the earth's superficial coatings.

[To be continued.]

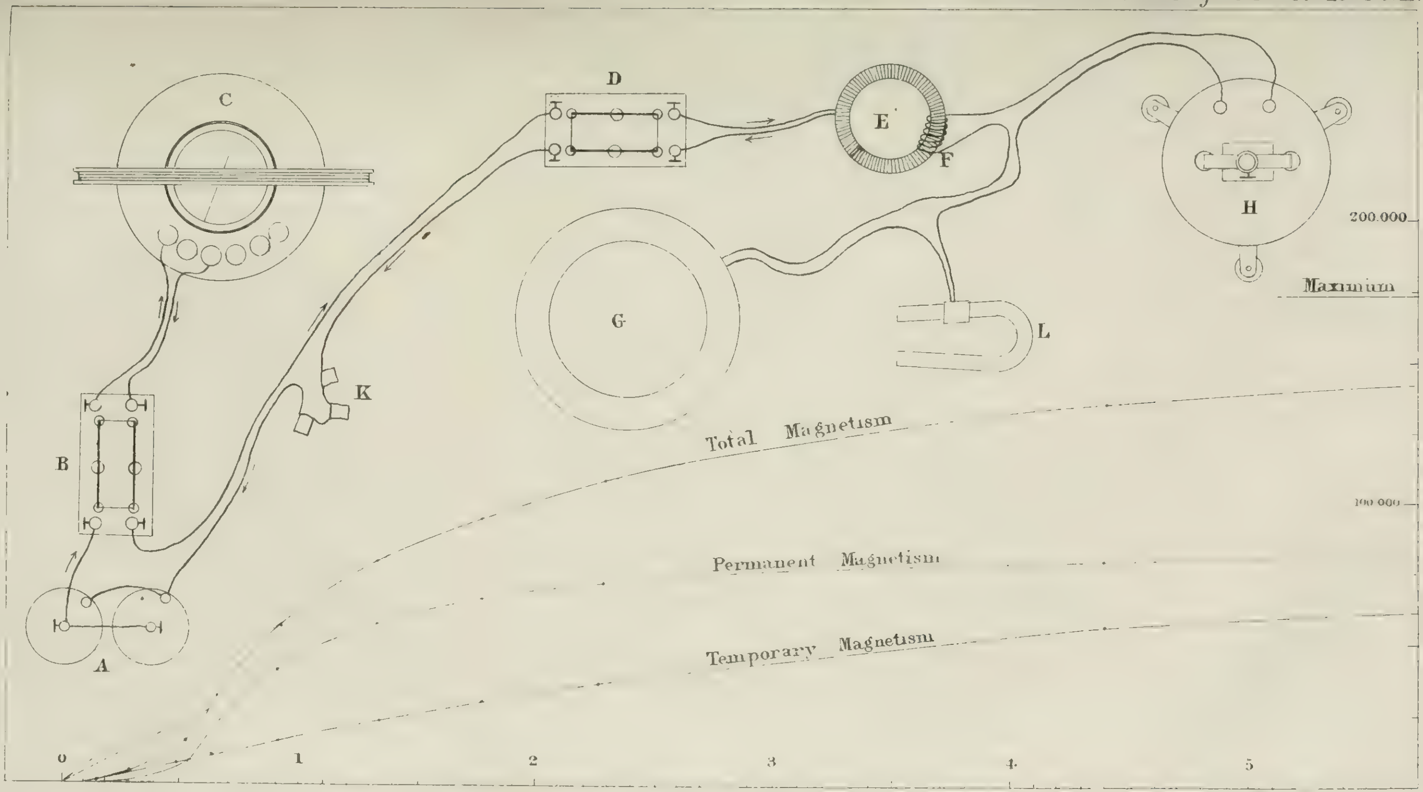
XIV. *On Magnetic Permeability*, and the Maximum of Magnetism of Iron, Steel, and Nickel.* By Henry A. Rowland, *Instructor in Physics in the Rensselaer Polytechnic Institute, Troy, N.Y.*†

[With Two Plates.]

MORE than three years ago I commenced the series of experiments the results of which I now publish for the first time. Many of the facts which I now give were obtained then; but, for satisfactory reasons, they were not published at that time. The investigations were commenced with a view to determine the distribution of magnetism on iron bars and steel

* The word "permeability" has been proposed by Thomson, and has the same meaning as "conductivity" as used by Faraday ('Papers on Electricity and Magnetism,' Thomson, p. 484; Maxwell's 'Electricity and Magnetism,' vol. ii. p. 51).

† Communicated by Professor J. Clerk Maxwell, M.A., F.R.S.



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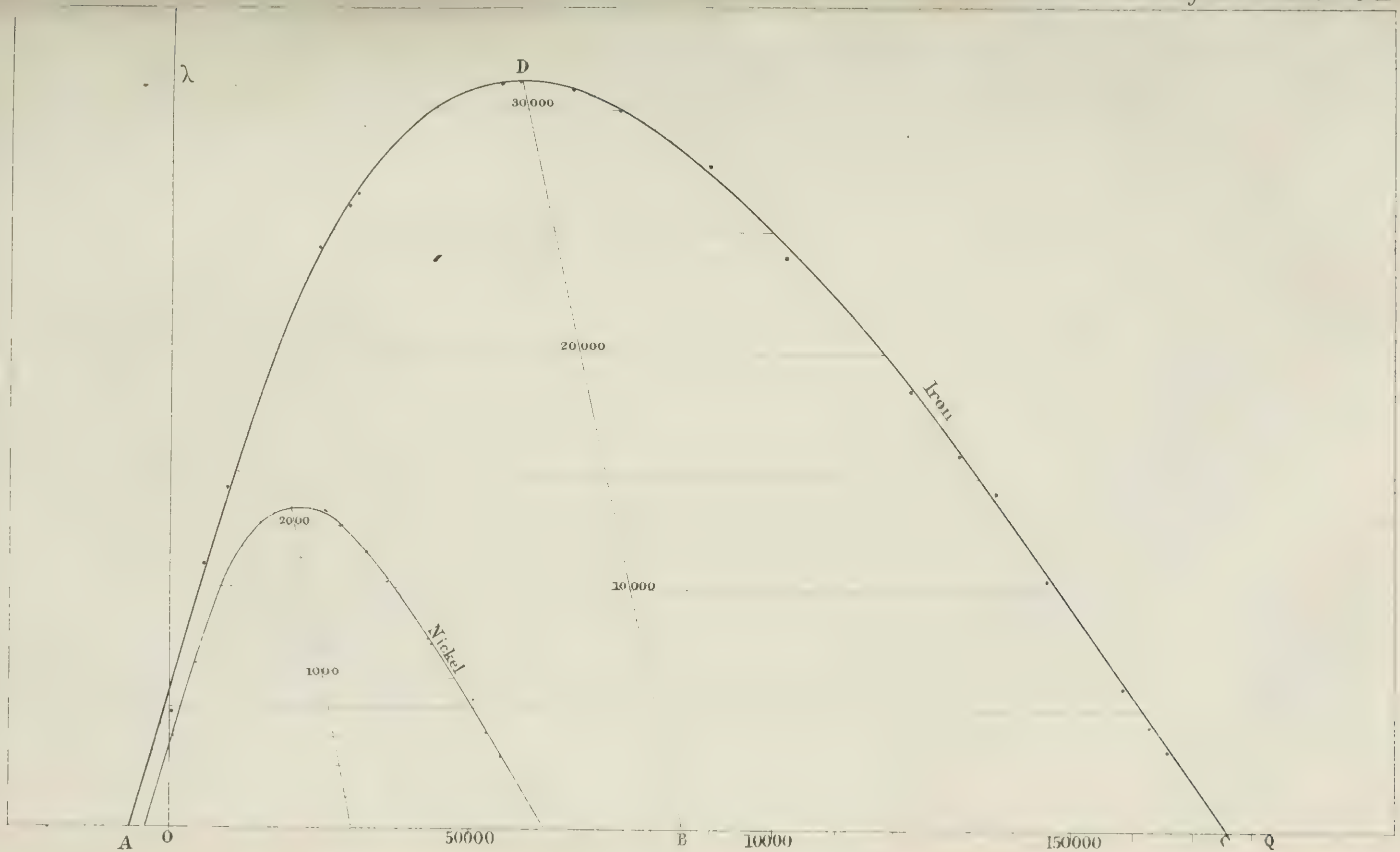


D
λ
30000
20000
10000
2000
1000
Iron
Nickel
A
0
50000
B
10000
150000
C
Q

magnets; but it was soon found that little could be done without new experiments on the magnetic permeability of substances.

Few observations have been made as yet for determining the magnetic permeability of iron, and none, I believe, of nickel and cobalt, in absolute measure. The subject is important, because in all theories of induced magnetism a quantity is introduced depending upon the magnetic properties of the substance, and without a knowledge of which the problem is of little but theoretical interest; this quantity has always been treated as a constant, although the experiments on the maximum of magnetism show that it is a variable. However, the form of the function has never been determined, except so far as we may deduce it from the equation of Müller,

$$\mathrm{I}=220d^{\frac{3}{2}}\tan\frac{m}{\cdot 00005d^2},$$

which, as will be shown, leads to wrong results. The quantities used by different persons are as follows:—

κ, Neumann's coefficient, or magnetic susceptibility (Thomson).

k, Poisson's coefficient.

μ, coefficient of magnetization (Maxwell), or magnetic permeability (Thomson).

λ, introduced for convenience in the following paper.

The relations of these quantities are given by the following equations:—

$$k=\frac{4\pi\kappa}{4\pi\kappa+3}=\frac{\mu-1}{\mu+2}=\frac{\lambda-4\pi}{\lambda+8\pi},$$

$$\kappa=\frac{\mu-1}{4\pi}=\frac{3k}{4\pi(1-k)}=\frac{\lambda-4\pi}{16\pi^2},$$

$$\mu=\frac{1+2k}{1-k}=4\pi\kappa+1=\frac{\lambda}{4\pi}.$$

The first determination of the value of any of these quantities was made by Thalén. But more important experiments have been made by Weber, Von Quintus Icilius, and more recently by M. Reicke and Dr. A. Stoletow*. The first three of these in their experiments used long cylindrical rods, or ellipsoids of great length; the last, who has made by far the most important experiments on this subject, has used an iron ring. The method of the ring was first used by Dr. Stoletow in September 1871; but more than eight months before that, in January 1871, I had used the same method, but with different apparatus, to measure the magnetism. He plots a curve showing the va-

* Phil. Mag. January 1873.

riation of κ; but he plots it with reference to R as abscissa instead of R κ, and thus fails to determine the law. His method of experiment is much more complicated than mine, so that he could only obtain results for one ring; while by my method I have experimented on about a dozen rings and on numerous bars, so that I believe I have been enabled to find the true form of the function according to which μ varies with the magnetism of the bar or the magnetizing-force.

Many experiments have been made on the magnetism of iron without giving the results in absolute measure. Among these are the experiments of Müller, Joule, Lenz and Jacobi, Dub, and others. The experiments have been made by the attraction of electromagnets, by the deflection of a compass-needle, or, in one case, by measuring the induced current in a helix extending the whole length of the bar. By the last two methods the change in the *distribution* of magnetism over the bar when the magnetism of the bar varies is disregarded, if indeed it was thought of at all: even in a recent memoir of M. Cazin* we have the statement made that the position of the poles is independent of the strength of the current. He does not give the experiment from which he deduces this result. Now it is very easy to show, from the formula of Green for the distribution of magnetism on a bar-magnet combined with the known variation of κ, that this can only be true for short and thick bars; and it has also been remarked by Thomson that this should be the case†. An experiment made in 1870 places this beyond doubt. A small iron wire (No. 16), 8 inches long, was wound with two layers of fine insulated wire; a small hard steel magnet $\frac{1}{4}$ inch long suspended by a fibre of silk was rendered entirely astatic by a large magnet placed about 2 feet distant; the wire electromagnet was then placed near it, so that the needle hung $1\frac{1}{2}$ inch from it and about 2 inches back from the end. On now exciting the magnet with a weak current, the needle took up a certain definite position, indicating the direction of the line of force at that point. When the current was very much increased, the needle instantly moved into a position more nearly parallel to the magnet, thus showing that the magnetism was now distributed more nearly at the ends than before. This shows that nearly all the experiments hitherto made on bar-magnets contain an error; but, owing to its small amount, we can accept the results as approximately true.

I believe mine are the first experiments hitherto made on this subject in which the results are expressed and the reasoning carried out in the language of Faraday's theory of lines of

* *Annales de Chimie et de Physique*, Feb. 1873, p. 171.

† Papers on Electricity and Magnetism, p. 512.

magnetic force; and the utility of this method of thinking is shown in the method of experimenting adopted for measuring magnetism in absolute measure, for which I claim that it is the simplest and most accurate of any yet devised. Whether Faraday's theory is correct or not, it is well known that its use will give correct results; at the present time the tendency of the most advanced thought is *toward* the theory*; and indeed it has been pointed out by Sir William Thomson that it follows, from dynamical reasoning upon the magnetic rotation of the plane of polarization of light, that the medium in which this takes place must itself be in rotation, the axis of rotation being in the direction of the lines of force†. Some substances must of necessity be more capable of assuming this rotary motion than others; and hence arises the notion of magnetic "conductivity" and "permeability."

Thomson has pointed out several analogies which may be used in calculating the distribution and direction of the lines of force under various circumstances. He has shown that the mathematical treatment of magnetism is the same as that of the flow of heat in a solid, as the static induction of electricity, and as the flow of a frictionless incompressible liquid through a porous solid. It is evident that to these analogies we may add that of the conduction of electricity‡. We readily see that the reason of the treatment being the same in each case is that the elementary law of each is similar to Ohm's law. Mr. Webb§ has shown that this law is useful in electrostatics; and I hope, in a sequel to this paper, to apply it to the distribution of magnetism: I give two equations derived in this way further on.

The absolute units to which I have reduced my results are those in which the metre, gramme, and second are the fundamental units. The unit of magnetizing-force of helix I have taken as that of one turn of wire carrying the unit current per metre of length of helix, and is 4π times the unit magnetic field. This is convenient in practice, and also because in the mathematical solution of problems in electrodynamics the magnetizing-force of a solenoid naturally comes out in this unit. The magnetizing-force of any helix is reduced to this unit by multiplying the strength of current in absolute units by the number of coils in the helix per metre of length. These remarks

* "On Action at a Distance," Maxwell, 'Nature,' Feb. 27 and March 6 and 13, 1873.

† Thomson's 'Papers on Electricity and Magnetism,' p. 419, note; and Maxwell's 'Treatise on Electricity and Magnetism,' vol. ii. chap. xxi.

‡ Maxwell's 'Treatise on Electricity and Magnetism,' arts. 243, 244, and 245.

§ "Application of Ohm's Law to Problems in Electrostatics," Phil. Mag. S. 4. vol. xxxv. p. 325 (1868).

apply only to endless solenoids, and to those which are very long compared with their diameter. The unit of number of lines of force I have taken as the number in one square metre of a unit field measured perpendicular to their direction. As my data for reducing my results to these units, I have taken the horizontal force of the earth's magnetism at Troy as 1·641, and the total force as 6·27.

The total force, which will most seriously affect my results, is well known to be nearly constant at any one place for long periods of time.

From the analogy of a magnet to a voltaic battery immersed in water I have obtained the following, on the assumption that μ is constant, and that the resistance to the lines of force passing out into the medium is the same at every point of the bar.

Let R = resistance to lines of force of one metre of length of bar.

R' = resistance of medium along 1 metre of length of bar.

Q' = lines of force *in* bar at any point.

Q_ϵ = lines of force passing from bar along small distance I.

ϵ = base of Napierian system of logarithms.

x = distance from one end of helix.

b = total length of helix.

s' = resistance at end of helix of the rest of bar and medium.

M = magnetizing-force of helix.

We then obtain

$$*\mathrm{Q}_\epsilon=\frac{\mathrm{MI}}{2\sqrt{\mathrm{RR}'}}\,\frac{1-\mathrm{A}}{(\mathrm{A}\epsilon^{rb}-1)}\,(\epsilon^{rx}-\epsilon^{r(b-x)}), \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\mathrm{Q}'=\frac{\epsilon^{rb}-1}{(\mathrm{A}\epsilon^{rb}-1)(\sqrt{\mathrm{RR}'}+s')}\,\frac{\mathrm{M}}{r}-\frac{\mathrm{M}}{2\mathrm{R}}\,\frac{1-\mathrm{A}}{\mathrm{A}\epsilon^{rb}-1}\,(\epsilon^{rb}+1-\epsilon^{rx}-\epsilon^{r(b-x)}), \quad ($$

in which

$$r=\sqrt{\frac{\mathrm{R}}{\mathrm{R}'}},$$

and

$$\mathrm{A}=\frac{\sqrt{\mathrm{RR}'}+s'}{\sqrt{\mathrm{RR}'}-s'};$$

for near the centre of an infinitely long bar, where $x>0$ and

* Formulæ giving the same distribution as this have been obtained by Biot and also by Green. See Biot's *Traité de Physique*, vol. iv. p. 669, and 'Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism,' by Green, 17th section.

$<b$, and $b=\infty$, we have

$$Q_\epsilon=0, \text{ and } Q'=\frac{M}{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

For a ring-magnet, $s'=0$;

$$\therefore Q_\epsilon=0, \text{ and } Q'=\frac{M}{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

And if a is the area of the bar or ring,

$$a\lambda=\frac{1}{R}=\frac{Q'}{M}, \text{ or } \lambda=\frac{Q'}{aM}, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

in which λ is the same as in the equations previously given. These equations show that we may find the value of λ, and hence the permeability, by experimenting either on an infinitely long bar or on a ring-magnet. Equations (4) evidently apply to the case where the diameter of the ring is large as compared with its section. The fact given by these equations can be demonstrated in another and, to some persons, more satisfactory manner. If n is the number of coils per metre of helix and n' the number on a ring-magnet, i the strength of current, and ρ the distance from the axis of the ring to a given point in the interior of the ring-solenoid, the magnetic field at that point will, as is well known, be

$$2n'i\frac{1}{\rho},$$

and at a point within an infinitely long solenoid

$$4\pi in.$$

If the solenoid contain any magnetic material, the field will be for the ring

$$2n'i\frac{\mu}{\rho}$$

and for the infinite solenoid

$$4\pi\mu in.$$

Therefore the number of lines of force in the whole section of a ring-magnet of circular section will be, if a is the mean radius of the ring,

$$Q'=4n'i\mu\int_{-R}^{+R}\frac{\sqrt{R^2-x^2}}{a-x}dx=4\pi n'i\mu(a-\sqrt{a^2-R^2});$$

or, since $n'=2\pi an$ and $M=in$, we have, by developing,

$$Q'=4\pi M\mu(\pi R^2)\left(1+\frac{1}{4}\frac{R^2}{a^2}+\frac{1}{8}\frac{R^4}{a^4}+\&c.\right). \quad . \quad . \quad (6)$$

For the infinite electromagnet we have in the same way for a circular section,

$$Q' = 4\pi M\mu(\pi R^2). \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

When the section of the ring is thin, equation (6) becomes the same as equation (7), and either of them will give

$$\lambda = 4\pi\mu = \frac{Q'}{(\pi R^2)M}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which is the same as equation (5).

In all the rings used the last parenthesis of (6) is so nearly unity that the difference has in most cases been neglected, the slightest change in the quality of the iron producing many times more effect on the permeability than this. Whenever the difference amounted to more than $\frac{1}{200}$ it was not rejected.

The apparatus used to measure Q′ was based upon the fact discovered by Faraday, that the current induced in a closed circuit is proportional to the number of lines of force cut by the wire, and that the deflection of the galvanometer-needle is also, for small deflections, proportional to that number. In the experiments of 1870–71 an ordinary astatic galvanometer was used; but in those made this year a galvanometer was specially constructed for the purpose. It was on the principle of Thomson's reflecting instrument, but was modified to suit the case by increasing the size of the mirror to $\frac{5}{8}$ of an inch, by adding an astatic needle just above the coil *without* adding another coil, by loading the needle to make it vibrate slowly, and, lastly, by looking at the reflected image of the scale through a telescope instead of observing the reflection of a lamp on the scale. The galvanometer rested on a firm bracket attached to the wall of the laboratory near its foundation. In most of the experiments the needle made about five single vibrations per minute. The astatic needle was added to prevent any external magnetic force from deflecting the needle; and directive force was given by the magnet above. Each division of the scale was ·075 inch long; and the extremities of the scale were reached by a deflection of 7° in the needle from 0. The scale was bent to a radius of 4 feet, and was 3 feet from the instrument. At first a correction was made for the resistance of the air &c.; but it was afterwards found by experiment that the correction was very exactly proportional to the deflection, and hence could be dispensed with. This instrument gave almost perfect satisfaction; and its accuracy will be shown presently.

The tangent-galvanometer was also a very fine instrument, and was constructed expressly for this series of experiments. The needle was 1·1 inch long, of hardened steel; and its deflec-

tions were read on a circle graduated to half degrees, and 5 inches in diameter. The average diameter of the ring was $16\frac{1}{2}$ inches nearly, and was wound with several coils; so that the sensibility could be increased or diminished at pleasure, and so give the instrument a very wide range. The value of each coil in producing deflection was experimentally determined to within *at least* $\frac{1}{3}$ of 1 per cent. by a method which I shall soon publish. The numbers to multiply the tangent of the deflection by, in order to reduce the current to absolute measure, were as follows:—

Number of coils.	Multiplier.
1	·05377
3	·01800
9	·006007
27	·002018
48	·001143

By this instrument I had the means of measuring currents which varied in strength several hundred times with the same accuracy for a large as for a small current. For greater accuracy a correction was applied according to the formula of Blanchet and De la Prevostaye for the length of the needle, the position of the poles being estimated; this correction in the deflections used was always less than ·6 per cent. To eliminate any error in the position of the zero-point, two readings were always taken with the currents in opposite directions, each one being estimated with considerable accuracy to $\frac{1}{10}$ of a degree.

The experiments were carried on in the assay laboratory of the Institute, which was not being used at that time; and precautions were taken that the different parts of the apparatus should not interfere with each other. The disposition of the apparatus is represented in Plate II.

The current from the battery A, of from two to six large Chester's "electropoion" cells No. 2, joined according to circumstances, passed to the commutator B, thence to the tangent-galvanometer C, thence to another commutator D, thence around the magnet E (in this case a ring), and then back through the resistance-coils K to the battery. To measure the magnetism excited in E, a small coil of wire F was placed around it*, which connected with the galvanometer H, so that, when the magnetism was reversed by the commutator D, the current induced in the coil F, due to twice cutting the lines of force of the ring, produced a sudden swing of the needle of H. As the needle swung very freely and would not of itself come to rest in ten or fifteen minutes, the little apparatus I was added: this consisted of

* If a bar was used, this coil was placed at its centre.

a small horseshoe magnet, on one branch of which was a coil of wire; and by sliding this back and forth, induced currents could be sent through the wire, which, when properly timed, soon brought the needle to rest. This arrangement was very efficient; and without it this form of galvanometer could hardly have been used. To compare the magnetism of the ring with the known magnetism of the earth, and thus reduce it to absolute measure, a ring G supported upon a horizontal surface was included in the circuit; when this was suddenly turned over, it produced an induced current, due to twice cutting the lines of magnetic force which pass through the ring from the earth's magnetism. The induced current in the case of either coil, F or G, is proportional to the number of the lines of force cut by the coils * and to the number of wires in the coil, which latter is self evident, but may be deduced from the law of Gaugain †. It is evident, then, that if c is the deflection from coil G, and h that from helix F, the number of lines of force passing through the magnet E, expressed in the unit we have chosen, will be

$$Q' = 2n'(6{\cdot}27 \sin 74^\circ\ 50')\pi R^2 \frac{1}{c}\frac{h}{2n}, \quad . \quad . \quad . \quad (9)$$

where n' is the number of coils in the ring G, n the number in the helix F, R the radius of G, 6·27 the total magnetism of the earth, and $74^\circ\ 50'$ the dip. The quantity $2n'\ (6{\cdot}27 \sin 74^\circ\ 50')\pi R^2$ is constant for the coil, and had the value 14·51. This is the number of square metres of a unit field which, when cut once by a wire from the galvanometer, would produce the same deflection as the coil when turned over.

The experiments being made by reversing the magnetism of the bars, a rough experiment was made to see whether they had time to change in half a single vibration of the needle; it was found that this varied from sensibly 0 to nearly 1 second, so that there was ample time. It was also proved that the sudden impulse given to the needle by the change of current produced the same deflection as when the change was more gradual, which has also been remarked by Faraday, though he did not use such sudden induced currents. As a test of the method, the horizontal force of the earth's magnetism was determined by means of a vertical coil; it was found to be 1·634, while the true quantity is 1·641.

It is sometimes assumed that some of the action in a case like the present is due to the direct induction of the helix around the magnet on the coil F. I think that this is not correct; for when

* Faraday's Experimental Researches, vol. iii. series 29.

† Daguin's *Traité de Physique*, vol. iii. p. 691.

the helix is of fine wire closely surrounding the bar or ring, all the lines of force which affect F must pass through the bar, and so no correction should be made. However, the correction is so small that it will hardly affect the result. If it were to be made, $\frac{Q'}{a}$ (equation 5) should be diminished by $4\pi M$; but, for the above reasons, it has not been subtracted. As a test of the whole arrangement, I have obtained the number of lines of force in a very long solenoid: the mean of two solenoids gave me

$$Q' = 12{\cdot}67M(\pi R^2);$$

while from theory we obtain, by equation (7) ($\mu = 1$),

$$Q' = 12{\cdot}57M(\pi R^2),$$

which is within the limits of error in measuring the diameter of the tubes &c.

All the rings and bars with which I have experimented have had a circular section. In selecting the iron, care must be used to obtain a homogeneous bar; in the case of a ring I believe it is better to have it welded than forged solid; it should then be well annealed, and *afterwards* have the outside taken off *all round* to about $\frac{1}{8}$ of an inch deep in a lathe. This is necessary, because the iron is "burnt" to a considerable depth by heating even for a moment to a red heat, and a sort of tail appears on the curve showing the permeability, as seen on plotting Table III. To get the normal curve of permeability, the ring must only be used *once*; and *then* no more current must be allowed to pass through the helix than that with which we are experimenting at the time. If by accident a stronger current passes, permanent magnetism is given to the ring, which entirely changes the first part of the curve, as seen on comparing Table I. with Table II. The areas of the bars and rings were always obtained by measuring their length or diameter across, and then calculating the area from the loss of weight in water. The following is a list of a few of the rings and bars used, the dimensions being given in metres and grammes. In the fourth column "annealed" means heated to a red heat and cooled in open air, "C annealed" means placed in a large crucible covered with sand, and placed in a furnace, where, after being heated to redness, the fire was allowed to die out; "natural" means that its temper was not altered from that it had when bought.

Results given in Table	Quality of substance.	How made.	Temper.	Spec. grav.	Weight.	Mean diam.	Area.	State.
I.	"Burden's best" iron.	Welded and turned.	Annealed.	7·63	148·61	·0677	·0000 916	Normal.
II.	" "	" "	"	7·63	148·61	·0677	916	Magnetic.
III.	" "	" "	C annealed.	7·63	148·01	·0677	912	Burnt.
IV.	Bessemer steel.	Turned from large bar.	Natural.	7·84	38·34	·0420	371	Normal.
V.	Norway iron.	Welded and turned.	C annealed.	7·83	39·78	·0656	7695	Magnetic.
VI.	Cast nickel*.	Turned from button.	......	8·83	4·806	·0200	0869	Normal.
VII.	Stubb's steel.	Hard-drawn wire.	Natural.	7·73	......	......	0969	Normal.

The first three Tables are from the same ring.

Besides these I have used very many other bars and rings; but most of them were made before I had discovered the effect of burning upon the iron, and hence did not give a normal curve for high magnetizing-powers. However, I have collected in Table VIII. some of the results of these experiments; but I have many more which are not worked up yet.

In the following Tables $Q=\frac{Q'}{\pi R^2}$ has been measured as previously described. It is evident that if, instead of reversing the current, we simply break it, we shall obtain a deflection due to the temporary magnetism alone. In this manner the temporary magnetism has been measured; and on subtracting this from Q, we can obtain the permanent magnetism.

The following abbreviations are made use of in the Tables, the other quantities being the same as previously described.

C.T.G. Number of coils of tangent-galvanometer used.
D.T.G. Deflection of tangent-galvanometer.
D.C. Deflection from coil G.
D.F. Deflection from helix F on reversing the current.
Q Magnetic field in interior of bar (total).
D.B. Deflection from F on breaking current.
T Magnetic field of bar due to temporary magnetism.
P Magnetic field of bar due to permanent magnetism.
n Number of coils in helix F.

$$Q=T+P.$$

* Almost chemically pure before melting.

Each observation given is almost always the mean of several. D.T.G. is the mean of four readings, two before and two after the observations on the magnetism; D.C. is the mean of from four to ten readings; D.F. mean of three; D.B. mean of two, except in Table I., where the deflection was read only once. In all these Tables the column containing the temporary magnetism T can only be accepted as approximate, the experiments having been made more to determine Q than T.

The value of n was generally varied by coiling a wire more or less around the ring, but leaving its length the same.

The change in the value of D.C. is due to the change in the resistance of the galvanometer from change of temperature, copper wire increasing in resistance about 1 per cent. for every 2°·6 C. rise. In Table I. the temperature first increased slowly, and then, after remaining stationary for a while, fell very fast.

Table I.

"Burden's Best" Iron, Normal.

C.T.G.	D.T.G.	M.	D.C.	n.	D.F.	$\frac{\text{D.F.}}{2n}$.	$\frac{\text{D.B.}}{n}$.	Q.	λ.	λ. Calculated.	$\mu=\frac{\lambda}{4\pi}$.	T.
48	4·5	·1456	23·4	30	6·5	·1083	·08	715	4910	5845	390·7	528
	16·45	·5501	...	...	54·6	·910	·59	6005	10920	10885	868·7	3894
	20·2	·6815	...	...	87·9	1·465	·80	9667	14180	14074	1129	5280
	28·6	1·011	23·3	10	74·2	3·71	1·34	24600	24330	24000	1936	8882
	31·1	1·119	...	...	88·2	4·41	1·48	29230	26120	26050	2078	9811
	31·9	1·155	...	...	92·6	4·63	1·53	30820	26690	26660	2124	10180
	41·12	1·623	...	2	29·8	7·45	2·0	49590	30570	30740	2433	13310
27	28·35	1·766	23·1	...	32·8	8·20	2·5	54820	31030	31050	2470	16710
	29·6	1·861	...	...	34·6	8·65	2·65	57820	31070	31100	2472	17710
	33·4	2·162	23·1	...	39·8	9·95	2·85	66510	30770	30776	2448	19050
	37·45	2·512	...	...	44·7	11·18	3·05	74730	29750	29930	2367	20390
	44·45	3·223	...	...	53·5	13·38	3·85	89430	27750	27390	2208	25740
	52·1	4·225	...	...	60·3	15·08	4·85	100800	23860	24730	1899	32420
9	34·65	6·744	...	...	73·1	18·28	7·10	122700	18210	18410	1448	47680
	39·8	8·136	23·0	...	77·3	19·32	7·90	129700	15940	16130	1269	53040
	44·3	9·542	...	1	40·6	20·30	9·1	136300	14280	13920	1137	61100
	55·1	14·04	...	...	43·5	21·75	9·8	145400	10360	10760	824·1	65510
3	42·95	27·18	...	...	47·4	23·70	11·5	157700	5803	6350	461·8	76540
	51·3	36·60	...	...	49·1	24·55	12·7	162700	4445	4523	353·8	84180
	60·15	51·18	23·4	...	50·3	25·15	13·2	166000	3243	3310	258·0	87120
		∞						175000		0		

Table II.

"Burden's Best" Iron, Magnetic.

M.	Q.	λ.	μ.	M.	Q.	λ.	μ.
·1456	426	2920	232	2·930	82720	28240	2247
·5699	3346	5987	476	4·210	100900	23950	1906
·6962	5700	8189	652	6·769	122800	18140	1444
1·080	24350	22550	1795	7·273	124300	17090	1360
1·191	29280	24580	1956	7·626	127100	16670	1326
1·537	46150	30020	2389	11·10	139500	12570	1000
1·590	49070	30260	2408	13·61	144700	10630	846
1·933	59680	30860	2456	22·10	154600	6965	554
2·377	71660	30150	2399				

Table III.

"Burden's Best" Iron, Burnt.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·143	1001	7039	560	1020	3·810	116900	30730	2446	
·553	9395	16980	1351	5115	4·283	120200	28060	2233	
·682	16550	24240	1929	6835	4·722	123900	26240	2088	30830
·962	37330	38780	3086	9454	6·565	133100	20270	1613	
1·070	42920	40130	3194	10300	9·326	141200	15140	1200	39810
1·153	48830	42340	3369	10530	11·00	144400	13120	1045	
1·317	59490	45180	3595	11650	13·44	147500	10970	873	44070
1·340	59580	44450	3538	13700	23·41	155500	6642	529	51030
2·127	90180	42400	3374	18470	32·73	159400	4870	387	
2·501	98560	39400	3136	19920	32·56	158400	4864	387	
2·864	104000	36310	2890	24600	51·03	165800	3250	259	56100
3·151	108200	34330	2732	24610					

Table IV.

Bessemer Steel, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1356	327	2412	192	309	2·756	39960	14500	1154	13080
·2793	817	2995	238	727	3·219	50550	15700	1250	16350
·5287	1726	3264	260	1471	3·551	56310	15860	1262	15980
·9398	3833	4079	325	3106	4·469	71380	15970	1271	18340
1·421	7702	5421	431	5576	5·698	85530	15010	1195	23610
1·880	14080	7487	596	8972	11·44	119550	10450	832	28020
1·947	15420	7920	630	8938	20 69	138300	6685	532	41360
2·300	24830	10800	859	11320	38·99	153700	3942	314	52930

TABLE V.

Norway Iron, Magnetic.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1344	865	6439	512		2·290	105900	46240	3680	35240
·2673	2550	9910	759	1892	4·393	134100	30520	2429	54970
·5161	13000	25200	2005	5857	5·910	142400	24090	1917	62810
·5572	15310	27480	2187	8110	7·874	149100	18940	1507	68490
·6725	30140	44820	3567	8921	13·77	156800	11390	906	77060
·9305	53800	57820	4602	13970	26·84	165800	6038	480	84710
1·362	77700	57110	4545	21630	36·86	168500	4572	364	87860
1·788	93000	52020	4140	28200					

TABLE VI.

Cast Nickel, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
1·433	852	595	47·4		13·43	27100	2018	160·6	11260
2·904	2377	819	65·1		16·53	31050	1878	149·5	13530
3·527	3685	1070	85·1		21·02	34950	1663	132·3	16480
5·555	10080	1815	144·4		32·17	41980	1305	103·8	22300
6·783	13680	2017	160·5	5120	33·92	42650	1257	100·0	23360
7·401	15270	2063	164·2	5614	60·91	50860	855	66·4	29540
9·273	19600	2114	168·2	7644	82·36	53650	651	51·8	33460
11·78	24720	2098	167·0	9902	105·2	55230	525	41·8	35120

TABLE VII.

Stubb's Steel Wire, Normal.

M.	Q.	λ.	μ.	T.	M.	Q.	λ.	μ.	T.
·1673	159	953	75·9		13·65	54300	3978	316·6	20900
·6237	678	1087	86·5	598	19·35	77770	4020	319·9	29480
1·084	1197	1104	87·9	1101	27·43	100800	3676	292·6	38590
2·043	2448	1199	95·4	2257	33·39	111300	3335	265·4	45110
2·714	3446	1270	101·0	3095	35·58	115000	3228	256·9	45950
4·221	6278	1487	118·4	5145	38·64	119400	3092	246·0	48060
10·26	33700	3286	261·5	16170					

The best method of studying these Tables is to plot them: one method of doing this is to take the value of the magnetizing-force as the abscissa, and that of the permeability as the ordinate; this is the method used by Dr. Stoletow; but, besides making the complete curve infinitely long, it forms a very irregular curve, and it is impossible to get the maximum of magnetism from it. Another method is to employ the same abscissas, but to use the magnetism of the bar as ordinates; this gives a regular curve, but has the other two disadvantages of the first method; however, it is often employed, and gives a pretty good

idea of the action. In Plate II. I have given a plot of Table V. with the addition of the residual or permanent magnetism, which shows the general features of these curves as drawn from any of the Tables. It is observed that the total magnetism of the iron at first increases very fast as the magnetizing-force increases, but afterwards more and more slowly until near the maximum of magnetism, where the curve is parallel to the axis of Q. The concavity of the curve at its commencement, which indicates a rapid increase of permeability, has been noticed by several physicists, and was remarked by myself in my experiments of January 1871; it has now been brought most forcibly before the public by Dr. Stoletow, whose paper refers principally to this point*. M. Müller has given an equation of the form

$$\mathrm{I}=220d^{\frac{3}{2}}\tan\frac{m}{\cdot 00005d^2}$$

to represent this curve; but it fails to give any concavity to the first part of the curve. A formula of the same form has been used by M. Cazin†; but his experiments carry little weight with them, on account of the small variation of the current which he used, this being only about five times, while I have used a variation in many cases of more than three hundred times.

Weber has obtained, from the theory that the particles of the iron are always magnetic and merely turn round when the magnetizing-force is applied, an equation which would make the first part of the curve coincide with the dotted line in Plate II.‡; and Maxwell, by addition to the theory, has obtained an equation which replaces the first part of the curve by the broken line§. I believe that I have obtained at the least a *very* close approximation to the true equation of the curve, and will show further on that Q and M must satisfy the equation

$$\frac{\mathrm{Q}}{\mathrm{M}}=\mathrm{A}\sin\left(\frac{\mathrm{Q}+a\dfrac{\mathrm{Q}}{\mathrm{M}}+\mathrm{H}}{\mathrm{D}}\right). \quad . \quad . \quad . \quad . \quad (10)$$

It is very probable that Weber's theory may be so modified as to give a similar equation.

Space will not permit me to discuss the curves of temporary

* On the Magnetizing-Function of Soft Iron, especially with the weaker decomposing powers. By Dr. A. Stoletow, of the University of Moscow. Translated in the Phil. Mag. January 1873. See particularly p. 43.

† *Annales de Chimie et de Physique,* February 1873, p. 182.

‡ This is according to Maxwell's integration of Weber's equation, Weber having made some mistake in the integration.

§ Treatise on Electricity and Magnetism, Maxwell, vol. ii. chap. vi.

and permanent magnetism; but I will call attention to the following facts which the Tables seem to establish.

1. *Nearly or quite all the magnetism of a bar is, with weak magnetizing-forces, temporary; and this is more apparent in steel than in soft iron.*

2. *The temporary magnetism increases continually with the current.*

3. *The permanent magnetism at first increases very fast with the current, but afterwards diminishes as the current increases, when the iron is near its maximum of magnetism.*

I have now described the methods of plotting the Tables hitherto used; and I will now describe the third, which is, I believe, new. This is by using the values of the magnetism of the bar as abscissas, and those of the permeability as ordinates. In this way we obtain a perfectly regular curve, which is of finite dimensions, and from which the maximum of magnetism can be readily obtained. Plate III. shows this method of plotting as applied to Table I. If we draw straight lines across the curve parallel to the axis of Q and mark their centres, we find that they always fall very exactly upon a straight line, which is therefore a diameter of the curve. The curve of nickel shown upon the same Plate has this property in common with iron. I have made several attempts to get a ring of cobalt; but the button has always been too porous to use. However, I hope soon to obtain one, and thus make the law general for all the magnetic metals. There are two equations which may be used to express the curve: one is the equation of an inclined parabola; but this fails for the two ends of the curve; the other is an equation of the general form

$$\lambda = \text{A} \sin\left(\frac{\text{Q} + a\lambda + \text{H}}{\text{D}}\right), \quad . \quad . \quad . \quad . \quad (11)$$

in which A, H, D, and a are constants depending upon the kind and quality of the metal used. A is the maximum value of λ, and gives the height of the curve E D, Plate III.; a establishes the inclination of the diameter; H is the line A O; and D depends upon the line A C. The following equation, adapted to degrees and fractions of a degree, is the equation from which the values of λ were found, as given in Table I.:

$$\lambda = 31{\cdot}100 \sin\left(\frac{\text{Q} + {\cdot}87\lambda + 5000}{1000}\right).$$

The large curve in Plate III. was also drawn from this, and the dots added to show the coincidence with observation; it is seen that this is almost perfect. As λ enters both sides of the equation, the calculation can only be made by successive ap-

proximations. We might indeed solve with reference to Q; but in this case some values of λ as obtained from experiment may be accidentally greater than A, and so give an imaginary value to Q.

By plotting any Table in this way and measuring the distance O C, we have the maximum of magnetism.

I have given in the same Plate the curve drawn from the observations on the nickel ring with Q on the same scale, but λ on a scale four times as large as the other. The curve of nickel satisfies the equation

$$\lambda = 2120 \sin\left(\frac{Q + 5{\cdot}35\lambda + 2000}{360}\right)$$

quite well, but not so exactly as in the case of iron. This ring, when closely examined, was found to be slightly porous, which must have changed the curve slightly, and perhaps made it depart from the equation.

In Table VIII. I have collected some of the values of the constants in the formula when it is applied to the different rings and bars, and have also given some columns showing the maximum of magnetism. When any blank occurs, it is caused by the fact that for some reason or other the observations were not sufficient to determine it. The values of a, H, D, and the value of λ, when $Q=0$, can in most cases only be considered approximate; for as they all vary so much, I did not think it necessary to calculate them *exactly*. For comparison, I have plotted Dr. Stoletow's curve and deduced the results given in the Table, of course reducing them to the same units as mine.

It will be observed that the columns headed "maximum of magnetism" contain, besides the maximum magnetic field, two columns giving the tension of the lines of force per square centimetre and square inch of section of the lines. These have been deduced from the formula given by Maxwell* for the tension per square metre, which is $\frac{Q^2}{4\pi}$ absolute units of force.

This becomes

$$\left.\begin{array}{l} \dfrac{Q^2}{2465500000} \text{ kilogrammes per square centim.,} \\[2ex] \dfrac{Q^2}{173240000} \text{ lbs. per square inch,} \end{array}\right\} \quad (12)$$

from which the quantities in the Table were calculated.

It is seen that the maximum of magnetism of ordinary bar iron is about 175,000 times the unit field, or 177 lbs. on the

* Treatise on Electricity and Magnetism, vol. ii. p. 256.

TABLE VIII.

Ring or bar.	Quality of substance.	Temper.	Maximum of magnetism. Magnetic field. Q.	Maximum of magnetism. Tension of lines in kil. per square centim.	Maximum of magnetism. Tension in lbs. on the square inch.	atest permea-bility. A.	atest permea-bility. μ.	*a*.	H.	D.	Spec. grav.	Value of λ when Q=0.	State of bar.
Ring.	Very fibrous wire ...	Annealed.	......	......	......	13900	1106	·60	......	......	7·51	1500	Burnt.
Bar.	Soft wire	,,	......	......	......	31000	2467	—·47	......	......	7·88	2300	,,
Ring.	"Burden's best" iron.	,,	175000	12·42	176·9	31100	2475	·87	5000	1000	7·63	4400	Normal.
Ring.	,, ,,	,,	175000	12·42	176·9	30900	2459	·87	3000	990	7·63	2700	Magnetic.
Ring.	,, ,,	Carefully annealed.	177000	12·71	180·8	45500	3621	·23	7000	*950	7·63	6800	Burnt.
Ring.	Norway	,,	181000	13·29	189·1	69300	5515	......	4000	......	7·87	9000	Normal.
Ring.	Norway	,,	174000	12·28	174·7	58500	4656	·344	3000	980	7·83	5500	Magnetic.
Ring.	Bessemer steel	Natural.	172000	12·00	170·8	16100	1281	1·10	6300	990	7·84	2500	Normal.
Bar.	Stubb's steel	,,	......	......	......	4160	331	2·45	10000	1010	7·73	950	,,
Bar.	"Burden's best" ...	,,	......	......	......	24800	1974	—·42	5000	......	7·65	3500	Burnt.
Bar.	,, ,,	Annealed.	......	......	......	27000	2149	—·56	6000	......	7·65	4800	,,
Ring.	Nickel	......	63000	1·61	22·9	2120	169	5·35	2000	360	8·83	470	Normal.
Ring.	Nickel	Carefully annealed.	64000	1·66	23·6	3830	305	......	1400	......	......	560	Burnt.
Ring.	Dr. Stoletow's ring.	Carefully annealed.	177000	12·71	176·9	27510	2190	·294	3300	......	......	2500	Magnetic.

* This satisfies all except the last few observations, which constitute the "tail" before referred to.

square inch, and for nickel 63,000 times, or 22·9 lbs. on the square inch. For pure iron, however, I think it may reach 180,000, or go even above that. It is seen that one of the Norway rings gave a very high result; this is explained by the following considerations. All the iron rings were welded except this one, which was forged solid from a bar 2 inches wide and then turned. Even the purest bar iron is somewhat fibrous; and between the fibres we often find streaks of scale lying lengthwise in the bar and so diminishing the section somewhat if the ring be welded from the bar; when, however, it is forged solid, these streaks are thoroughly disintegrated; and hence we find a higher maximum of magnetism for a ring of this kind, and one approaching to that of pure iron. But a ring made in this way has to be exposed to so much heating and pounding that the iron is rendered unhomogeneous, and a tail appears to the curve like that in Table III. It is evident that this tail must always show itself whenever the section of the ring is not homogeneous throughout.

Hence we may conclude that the greatest weight which can be sustained by an electromagnet with an infinite current is, for good but not pure iron, 354 lbs. per square inch of section, and for nickel 46 lbs.

Joule* has made many experiments on the maximum sustaining-power of magnets, and has collected the following Table, which I give complete, except that I have replaced the result with his large magnet by one obtained later.

Table IX.

Magnet belonging to		Least area of section, square inch.	Weight sustained.	Weight sustained ÷ least area	Q.
Mr. Joule.	1.	10	2775	277	154700
	2.	·196	49	250	147000
	3.	·0436	12	275	154100
	4.	·0012	·202	162	118300
Mr. Nesbit......		4·5	1428	317	165500
Prof. Henry ...		3·94	750	190	128200
Mr. Sturgeon...		·196	50	255	148500

It is seen that these are all below my estimate, as they should be. For comparison, I have added a column giving the values of Q which would give the sustaining-power observed; some of these are as high as any I have *actually* obtained, thus giving an experimental proof that my estimate of 354 lbs. cannot be far from correct, and illustrating the beauty of the absolute

* Phil. Mag. 1851.

system of electrical measurement by which, from the simple deflection of a galvanometer-needle, we are able to predict how much an electromagnet will sustain without actually trying the experiment.

In looking over the columns of Table VIII. which contain the values of the constants in the formula, we see how futile it is to attempt to give any fixed value to the permeability of iron or nickel; and we also see of how little value experiments on any one kind of iron are. Iron differs as much in magnetic permeability as copper does in electric conductivity.

It is seen that in the three cases when iron bars have been used, the value of a is negative; we might consider this to be a general law, if I did not possess a ring which also gives this negative. All these bars had a length of *at least* 120 times their diameter.

The mathematical theory of magnetism has always been considered one of the most difficult of subjects, even when, as heretofore, μ is considered to be a constant; but *now*, when it must be taken as a function of the magnetism, the difficulty is increased many fold. There are certain cases, however, where the magnetism of the body is uniform, which will not be affected.

Troy, June 2, 1873.

XV. *On Objections recently made to the received principles of Hydrodynamics.* *By* Professor CHALLIS, *M.A., LL.D., F.R.S., F.R.A.S.**

A LARGE proportion of my scientific labours having been devoted to researches respecting the principles and applications of Hydrodynamics, I have naturally taken much interest in the discussion carried on in this Magazine between Mr. Moon and the Hon. J. W. Strutt (Lord Rayleigh) relative to points affecting the very foundations of that department of science. After considering the arguments on both sides, it appears to me that the questions raised demand additional elucidation, to supply which is the purpose of this communication.

Mr. Moon's equation (1) in page 117 of vol. xxxvi. of the Philosophical Magazine, viz.

$$0=\frac{d^2y}{dt^2}+\frac{1}{\mathrm{D}}\frac{dp}{dx},$$

includes the principle of constancy of mass, as is shown in the Note in p. 101 of vol. xlv., and for motion in one dimension is perfectly general. Respecting that equation Mr. Moon remarks

* Communicated by the Author.

that "it holds without reference to any hypothesis as to the law of pressure." I should rather say that it involves no specification of a particular fluid, and is therefore applicable to all perfect fluids.

I see nothing to object to in the argument which follows (in p. 117), from which Mr. Moon comes to the general conclusion that the pressure (p) is a function of the density (ρ) and velocity (v). This theorem, which, as far as I know, Mr. Moon has the merit of first demonstrating, is in the same sense general as the equation (1); that is, it contains no specification of the fluid. It shows in fact that, in defining the particular fluid, we are limited to the hypothesis that the pressure is some function of the density and the velocity. Now evidently this general theorem includes the more particular case in which the pressure is a function of the density only. Therefore, by Mr. Moon's own showing, it is legitimate to make the *hypothesis* that the pressure of the fluid is always in exact proportion to its density, and by this supposition to *define* the fluid. This argument proves, since it wholly depends on the fluid being in a state of motion, that the equation $p=a^2\rho$ may be assumed relatively to *fluid in motion*, and that it is not necessary, if it were possible, to establish its truth for that case by direct experiment.

Mr. Moon is evidently not aware that such an inference is deducible from his argument, inasmuch as he proceeds to employ the equation (1) and the theorem $p=$ funct. (ρ and v) in all their generality, as if no other course of reasoning were legitimate; and in this manner he obtains expressions for the pressure, density, and velocity which satisfy the general equation (1) (see Phil. Mag. vol. xxxvi. p. 124). According to the view I have indicated, these expressions embrace the values of the pressure, density, and velocity for every particular fluid comprehended under the general definition $p=$ funct. (ρ, v). I have found, in fact, that they are inclusive of the values obtained for air of given temperature in the ordinary way from the equation $p=a^2\rho$.

But after proving analytically that it is allowable to assume that $p=a^2\rho$ for fluid in motion, the question as to whether this equation is true for a particular fluid (as the air) is, as Lord Rayleigh rightly urges, a *physical* one, and can only be settled by the aid of experiment. The proper process for this purpose is to introduce into the general differential equations applicable to the motion of a fluid the relation $p=a^2\rho$ (already shown to be, *pro hac vice*, legitimate), and then, after applying the integrals of the equations in solving various hydrodynamical problems, to compare numerically the results of the solutions with experiment. Such evidence of the exactness of the equation $p=a^2\rho$ is *accumulative*, increasing with the number and variety

of the comparisons that admit of being made satisfactorily. One contradictory experiment would be fatal to its truth. As far as my acquaintance with the solutions of hydrodynamical problems extends, I have seen no reason to doubt the exactness of that equation for air of given temperature in motion, within a wide range of density.

The legitimacy of the hypothesis that $p=a^2\rho$ having been proved by an argument for which Mr. Moon is himself responsible, it must be under some misapprehension that he contends by other arguments that that equation is not admissible. The fact is, these arguments are vitiated by the circumstance that inferences, drawn from an investigation which embraces perfect fluids of all kinds, are treated as applicable to a particular fluid without previously introducing any condition defining the fluid.

To illustrate the foregoing views, I propose to discuss an instance of the application of the equation $p=a^2\rho$ which is considered by Mr. Moon to lead to absurd conclusions, viz. that in which "a vertical cylinder closed at its lower end has an air-tight piston, which is capable of working freely in the upper part of it, and is exactly supported by the air beneath; and at a given time a given weight is placed upon the piston." The question is, to determine what motion of the piston is thereby produced. From what has been already said, it will be assumed that the equation $p=$ funct. (ρ and v) justifies making the hypothesis that $p=a^2\rho$ for *air in motion*; and the solution of the problem will accordingly proceed as follows.

Let the weight of the piston be Mg, and the additional weight mg; and let the distance of the under surface from the bottom of the cylinder be a when the weight is added, and z_1 after the time t. Also let k^2p_0 be the whole pressure of the fluid upon the piston before the weight is put on, and k^2p_1 that at the time t. In general, if p be the pressure at any point of the fluid at the height z above the bottom of the cylinder at any time t, we shall have $p=$ funct. (z, t), and therefore

$$\left(\frac{dp}{dt}\right)=\frac{dp}{dt}+\frac{dp}{dz}\frac{dz}{dt}.$$

The factor $\frac{dp}{dz}$, expressing the variation of the pressure from point to point at a given time, depends partly on the circumstance that the impulse immediately given by the bottom of the piston to the contiguous fluid is *propagated*, so that for any other position the consequent pressure is a function of z and t, and partly on the variation of the pressure at the given time due to the force of gravity. As the total variation of pressure referable to these two causes will always be comparatively very small for

a cylinder of moderate dimensions, for the sake of simplicity it will be neglected, and the density of the fluid will be assumed to be at each instant the same throughout, and to vary only with the time. Accordingly for the acceleration of the piston we have

$$\frac{d^2z_1}{dt^2}=\frac{k^2p_1}{\mathrm{M}+m}-g;$$

and since by hypothesis $k^2p_0=\mathrm{M}g$, and $\frac{k^2p_1}{k^2p_0}=\frac{a}{z_1}$, it follows that

$$\frac{d^2z_1}{dt^2}=\frac{\mathrm{M}ag}{(\mathrm{M}+m)z_1}-g.$$

We may now suppose, in order to make the problem more general, that the mass m impinges on the mass M of the piston with a given velocity V, and that the two masses subsequently move on together. Hence their initial common velocity is $\frac{m\mathrm{V}}{\mathrm{M}+m}$. In this expression so much of the momentum of the fluid as is generated by the impact *at the first instant* is left out of account as being indefinitely small. The same is the case with respect to the impact of one *solid* body on another, if impact be regarded as a short and violent pressure; for the momentum of the impinging body is altered *by degrees*, beginning with zero, by the reaction of the other, and it is after an interval, however short, that the permanent alteration of its momentum is effected. In the problem before us, the reaction of the fluid is taken account of with sufficient approximation by supposing the density of the air and its pressure on the piston to be *at all times* inversely proportional to the space which the air occupies. This being admitted, the integration of the above equation gives

$$\frac{dz_1^2}{dt^2}=\frac{m^2\mathrm{V}^2}{(\mathrm{M}+m)^2}+2g(a-z_1)+\frac{2\mathrm{M}ga}{\mathrm{M}+m}\log\frac{z_1}{a}.$$

This equation includes the effect of the impact of the momentum $m\mathrm{V}$, and is applicable whether V is positive or negative. The case of V negative might be practically exemplified by suddenly attaching the mass m to M by a string passing over a pulley. In that case z_1 is always greater than a.

If $\mathrm{M}=0$, the formula gives for the square of the velocity of m, $\mathrm{V}^2+2g(a-z_1)$, which is the same as that for motion in free space, as it ought to be. For since $k^2p_0=\mathrm{M}g$, if $\mathrm{M}=0$, we should have $p_0=0$, and there would be no pressure to impede the descent of m.

If $\mathrm{V}=0$, or the mass m be added to M without impact, the

expression for the square of the velocity of the piston becomes

$$2g(a-z_1)+\frac{2\mathrm{M}ga}{\mathrm{M}+m}\log\frac{z_1}{a}.$$

This expression is still applicable if m be a negative quantity—that is, if the mass M of the piston be supposed to be suddenly diminished by the quantity m. In that case z_1 is greater than a, and the velocity becomes zero for the value of z_1 which satisfies the equation

$$2g(z_1-a)=\frac{2\mathrm{M}ga}{\mathrm{M}-m}\log\frac{z_1}{a}.$$

If we suppose that $m=-\mathrm{M}$, which is to suppose that the surface of the fluid is exposed to vacuum by a sudden abstraction of the piston, the above expression for the square of the velocity fails to give any definite result, because the factor $\frac{2\mathrm{M}ga}{\mathrm{M}+m}$ becomes infinite. This case is referred to by Mr. Moon at the end of his article in the Philosophical Magazine for February 1873; and a similar one is adduced in p. 27, vol. xxxvi., where a finite density D is assumed to be immediately contiguous to a density 2D. Such instances of changes of the density, and consequently of the pressure, *per saltum*, are not embraced by the ordinary principles of analytical hydrodynamics; if susceptible of treatment, they would require the application of new principles. I do not profess to be able to indicate what would be the appropriate process; but I regard it as certain that no argument against the validity in general of the equation $p=a^2\rho$ can be drawn from exceptional cases in which the density and pressure vary *per saltum*.

The general expression for the acceleration of the piston *downwards* being

$$g-\frac{\mathrm{M}ag}{(\mathrm{M}+m)z_1},$$

at the first instant, when $z_1=a$, this becomes $g-\frac{\mathrm{M}g}{\mathrm{M}+m}$, or $\frac{mg}{\mathrm{M}+m}$. The piston begins to descend by the action of this force, for the same reason that a free body begins to descend by the action of gravity; and, as Lord Rayleigh justly maintains, any argument adduced to prove that the descent cannot commence in the former case must equally apply in the other. I think I have sufficiently shown that Mr. Moon's dissent from this view is attributable to his misunderstanding the character of the equation $p=$ funct. (ρ and v).

I propose to conclude this communication by making some

remarks on the parts respectively performed by *reasoning* and by *experiment* or *observation,* in establishing the truth of the results obtained in applied mathematics, not having seen in modern scientific productions sufficiently precise information as to the relation between these two parts.

(1) The experimental results obtained by Galileo and Atwood respecting the vertical descent of bodies acted upon by gravity, and the experimental proof that the vertical acceleration of a projectile is independent of the motion in a curvilinear path, and and that its horizontal motion is uniform, establish the laws which govern *constant* accelerating forces. It was absolutely necessary to ascertain these laws by *experiment* before any *abstract reasoning* respecting accelerating force was possible. But the laws relating to constant accelerating forces having thus become known, it can be proved by means of Taylor's theorem, or some equivalent process, that an accelerating force, whether variable or constant, estimated in a given direction, is quantitatively expressed by *the second differential coefficient* of the function which gives the distance at any time of the accelerated particle from a fixed plane perpendicular to that direction. Taylor's theorem is legitimately employed for this purpose on the general principle that abstract calculation is comprehensive of all concrete physical relations. This, in fact, is the proper *à priori* argument for the truth of that general symbolic expression for accelerative force.

On the same expression the whole of Physical Astronomy depends, as well as every department of applied science which requires the calculation of motions produced by accelerative forces. The satisfactory comparison of the results of calculation so made with observed facts constitutes the argument *à posteriori* for the truth of the analytical symbol for accelerative force, and with so much the greater evidence as the number of such comparisons is greater. The analytical proof of Kepler's laws is a very important part of such evidence, inasmuch as it involves as a corollary the demonstration of the law of gravity, which could not be given by observation alone, nor by calculation alone, but requires that the two processes be combined. In short, the discovery by geometrical or analytical *reasoning* that a variable force is expressible by the second differential coefficient of space with respect to time, specially characterizes the Newtonian epoch of physical science.

(2) A perfect fluid being defined by the properties of mutual pressure, and of easy separability, of contiguous parts, it is possible to demonstrate, for fluid at rest, the law of equal pressure in all directions from a given point, exclusively by *reasoning* founded on these properties. (See 'Principles of Mathematics

and Physics,' pp. 104–107.) In certain cases the law admits of being verified directly by *experiment*; but generally the verification is effected by comparisons of results obtained mathematically, on the hypothesis of the truth of the law, with observed facts. Also for the case of fluid in motion an *à priori* demonstration of the same law may be given on principles which are indicated in pp. 172 and 173 of the work just cited. But in this case experiment cannot be directly employed for its verification; and on this account some mathematicians have thought that the law was not as certainly established for fluid in motion as for fluid in equilibrium. It may, however, be asserted that, although for fluid in motion the demonstration can only be effected by the combination of reasoning with the results of experiment, both the *à priori* proof of the law, and its verification *à posteriori* by comparison of calculated results with observation, are equally valid whether the fluid is in motion or at rest.

(3) Assuming that Boyle's law has been proved by experiment to be true for air of given temperature at rest, what evidence is there that it is true for air in motion? This question, to which, as far as I am aware, a satisfactory answer has not hitherto been given, I propose to answer as follows:—Since it does not appear practicable to verify the law for fluid in motion by direct experiment, the only course that can be adopted is to justify the assumption of the law for that case by *à priori* reasoning. This having been done, the truth of the assumption has to be confirmed, as in all like instances, by comparison of results mathematically deduced from it with experiment. The chain of the reasoning is therefore incomplete unless it is shown by an *à priori* argument that the equation $p=a^2\rho$ is not excluded by the state of motion. Now this link is supplied by Mr. Moon's equation $p=$ funct. (ρ and v), the investigation of which rests on the general principle that all physical relations expressible by functions of space and time are comprehended by the rules of abstract calculation. Thus that equation is quite general, including every supposable relation between ρ and v; and accordingly we may assume that v vanishes from the equation, or that v is a function of ρ. In either case p becomes a function of ρ only. Now, since in the original investigation ρ was assumed to be a function of space and time, this relation between p and ρ, including as a particular case $p=a^2\rho$, must apply to fluid in motion. Hence Boyle's law may be assumed as a basis of reasoning in hydrodynamics.

XVI. *On the Nodal Lines of a Square Plate.*

By Lord RAYLEIGH, *F.R.S.* (Hon. J. W. STRUTT)*.

IN a recent Number of Poggendorff's *Annalen*† there is a short paper by Strehlke, complaining of the inaccuracy with which the nodal lines of vibrating square plates are depicted in certain elementary works, and drawing attention to the results of his own careful measurements. His remarks relate principally to that mode of vibration which has for nodal line a closed curve not greatly differing from a circle, but which has been erroneously regarded by some as a slightly modified form of the inscribed square.

On the experimental determination of nodal lines there is no greater authority than Strehlke; and if there were no light from theory, his results might be considered to exhaust the question. Strehlke, indeed, quotes Professor Kirchhoff as expressing the opinion that there is at present no prospect at all of a theoretical solution of the problem—an opinion which may be correct if understood to refer to a *perfectly general* theory, but which is certainly erroneous if intended to apply to the particular mode of vibration under discussion. So long ago as 1833 Wheatstone pointed out the right path ‡, though he did not follow it correctly. He considers the vibration as resulting from the superposition of two independent but equal and synchronous vibrations, in each of which the plate vibrates according to the same law as a simple bar, each line of the plate parallel to one pair of edges being affected by the same motion. On account of the symmetry, the period is the same, whichever pair of edges be taken; and thus the two vibrations compound into one having the same period, whatever may be the ratio of amplitudes. In the present case the two vibrations must be considered to have the same phase and equal amplitudes, so that the motion at the centre of the square is simply doubled. The nodal line is then the locus of points at which the component vibrations neutralize each other.

This view is perfectly correct; but Wheatstone went wrong in his application of it, from not sufficiently considering what the law of vibration of a bar really is. His conclusion that the nodal line coincides with the inscribed square involves the suppositions that when a bar vibrates freely (in its gravest mode) the amplitudes at its centre and ends are numerically equal, and that the nodes lie midway between these points. As a matter of fact neither of these suppositions is correct; the amplitude

* Communicated by the Author.

† Pogg. *Ann.* vol. cxlvi. p. 319.

‡ Phil. Trans. 1833.

of vibration at the ends is decidedly (more than in the ratio of 3 : 2) greater than at the centre, from which it follows that the nodal line does not pass through the centres of the sides, and the nodes of a vibrating bar deviate sensibly from the positions assigned to them. It will be observed that the four points in which the required locus intersects the diagonals of the square are determined by a knowledge of the nodes of the component vibrations; for a point which lies on both these systems necessarily satisfies the condition for the nodal line of the resultant vibration. Further it is obvious that no part of the nodal line can lie in those compartments of the plate where the component vibrations have the same sign, whether positive or negative; that is to say, the curve passes through the rectangles, and not through the squares, into which the disk is divided by the two primary systems of nodes. The reproduction of Wheatstone's argument in a work so well known as Tyndall's 'Lectures on Sound' relieves me from the necessity of stating it at greater length here.

The algebraical calculation of the form of a bar vibrating freely was given originally by Euler; but I do not find that the result has been reduced to numbers further than was necessary for calculating the position of the nodes. If the distance of any point from the end, expressed as a fraction of the total length, be x, and z denote the transverse displacement, we have the following expression, giving the relative displacements of the different points of the bar*:

$$z = -\sqrt{2}\sin\left(\frac{180mx}{\pi} - \frac{180\beta}{2\pi} - 45\right)^0$$

$$+\epsilon^{mx}\sin\left(\frac{180\beta}{2\pi}\right)^0 + \epsilon^{-mx}\cos\left(\frac{180\beta}{2\pi}\right)^0; \quad . \quad . \quad . \quad . \quad (1)$$

where, approximately,

$$m = 4{\cdot}7300, \quad \beta = {\cdot}0176. \quad . \quad . \quad . \quad . \quad . \quad (2)$$

For numerical computation put $x = \frac{r}{20}$; then on reduction we have z expressed as the sum of three terms, the first of which is $-\sqrt{2}\sin(r \times 13^\circ\ 33' - 45^\circ\ 30'\ 15'')$, and the logarithms of the two others $r \times {\cdot}10271 + \bar{3}{\cdot}9444$ and $-r \times {\cdot}10271 + \bar{1}{\cdot}99998$ respectively.

From this formula z was calculated for integral values of r from 0 to 10, and the results reduced in such a proportion as to make the last term (that corresponding to the middle of the rod)

* Donkin's 'Acoustics,' p. 190. More exact values are $m = 4{\cdot}73004$, $\beta = {\cdot}01765$.

equal to unity. The intervals were then bisected by means of the appropriate interpolation-formulæ involving the use of four terms. If p, q, r, s be four consecutive values of z, the interval between q and r is bisected by

$$z=\frac{q+r}{2}+\frac{q+r-(p+s)}{16} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

At the ends of the Table a modification is required. For the term next before that corresponding to the middle of the rod we have $s=q$, as we know that every thing is symmetrical about the centre. For the term corresponding to $x=0{\cdot}025$, I have contented myself with a simple interpolation by first differences, a course justified by the fact that in the neighbourhood of the ends the curvature is exceedingly small. The Table stands as follows:—

x.	z.	Diff.	x.	z.	Diff.
·000	−1·6448		·250	+·1632	·1577
·025	1·4538	·1910	·275	·3110	·1478
·050	1·2628	·1910	·300	·4475	·1365
·075	1·0724	·1904	·325	·5713	·1238
·100	·8835	·1889	·350	·6814	·1101
·125	·6966	·1869	·375	·7765	·0951
·150	·5131	·1835	·400	·8559	·0794
·175	·3340	·1791	·425	·9183	·0624
·200	− ·1607	·1733	·450	·9636	·0453
·225	+ ·0055	·1662	·475	·9908	·0272
			·500	1·0000	·0092

It appears that the displacement at the ends of the rod is by no means of the same numerical magnitude as at the middle. It will be found by interpolation that $z=-1$ when $x=\cdot 0846$, which last number therefore gives the nearest approach made by the nodal line of the resultant vibration to the sides of the square. The nodal lines of the original systems correspond to $x=\cdot 2242$, differing sensibly from $\frac{1}{4}$.

If we take two adjacent sides of the square as axes of Cartesian coordinates, the lines of constant displacement are represented by

$$z_x+z_y=\text{const.}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

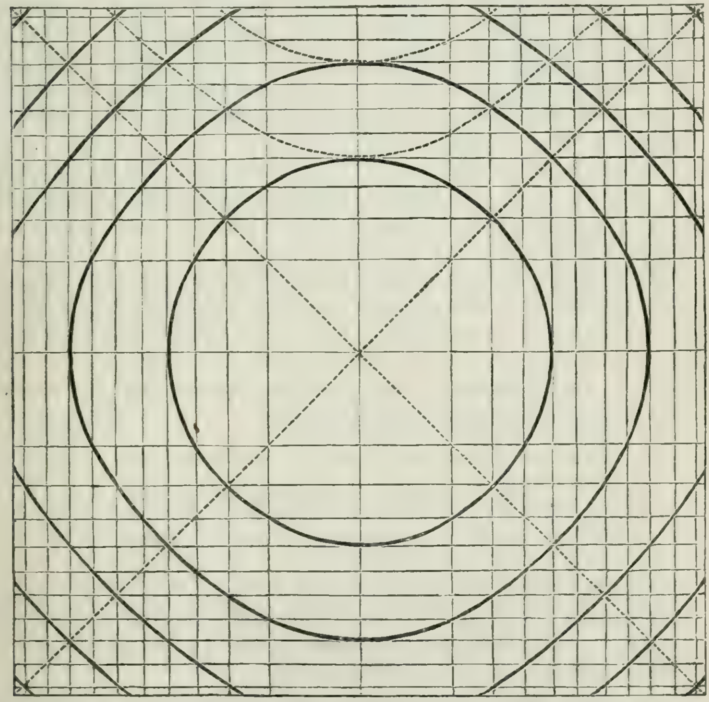
z being the function already investigated and tabulated. For the nodal line the constant in (4) is to be put equal to zero.

In order to construct (4) graphically, the most convenient method is that adopted by Maxwell in similar cases. The systems of curves (in this case straight lines) represented by $z_x=$ const. and $z_y=$ const. respectively are first laid down, the values of the constants forming an arithmetical progression with the same common difference in the two cases. In this way a

network is obtained which the required curves cross diagonally. The execution of this plan requires an inversion of the Table connecting z and x, of which the result is as follows:—

z.	x.	Diff.
$+1·00$	·5000	
·75	·3680	·1320
·50	·3106	·0574
·25	·2647	·0459
·00	·2242	·0405
$-$ ·25	·1871	·0371
·50	·1518	·0353
·75	·1179	·0339
1·00	·0846	·0333
1·25	·0517	·0329
$-1·50$	·0190	·0327

The system of lines represented by the above values of x (completed symmetrically on the further side of the central line) and the corresponding system for y are laid down in the figure.



From these the curves of equal displacement are deduced. At the centre of the square we have z a maximum, and equal to 2

(on the scale adopted). The first curve proceeding outwards is the locus of points at which $z=1$. The next is the nodal line, separating the regions of opposite displacement. The remaining curves taken in order give the displacements -1, -2, -3. The numerically greatest negative displacement does not occur until the corners of the square, where it amounts to $2\times 1{\cdot}6448 = 3{\cdot}2896$.

The same calculations and constructions give the theoretical solution for another and even better-known mode of vibration of a square plate. We have hitherto supposed that the two component vibrations were in the same phase. If we take them in opposite phases, the curves of equal displacement become

$$z_x - z_y = \text{const.}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \qquad (5)$$

which are to be found by crossing the same network of straight lines along the other diagonals. In this mode of vibration the nodal lines are the diagonals of the square; and the hyperbolic curves in the figure (when completed on the opposite side) are the loci of points where the displacement amounts respectively to 1 and 2. The curves similarly situated in the other two portions cut off from the square by the diagonals would correspond to displacements -1 and -2. The maxima of vibration occur at the middle points of the sides of the square, and amount numerically to $1+1{\cdot}6448=2{\cdot}6448$.

Other modes of vibration of a square plate may be obtained by starting from the higher fundamental modes of a bar with three or more nodes. Some of these I hope to examine in my work on Acoustics, now in preparation.

Strehlke expresses the form of the nodal line as given by his observations by means of polar equations, the origin being taken at the centre of the square. This was a natural course to take in view of the approximate circular form, but does not commend itself to the theorist. The equations representing the results obtained with three different plates are

$$r = {\cdot}40143 + {\cdot}0171 \cos 4t + {\cdot}00127 \cos 8t,$$

$$r = {\cdot}40143 + {\cdot}0172 \cos 4t + {\cdot}00127 \cos 8t,$$

$$r = {\cdot}4019 \;\; + {\cdot}0168 \cos 4t + {\cdot}0013 \;\; \cos 8t.$$

From these we obtain for the radius vector parallel to the sides of the square ·41980, ·41981, ·4200, while the theoretical result is ·4154. The radius vector measured along a diagonal is ·3856, ·3855, ·3864, but from theory ·3900.

The agreement between theory and observation is not so good as might have been expected from the case of a circular plate, for which the theoretical results were calculated by Poisson and

Kirchhoff, and tested experimentally by Strehlke; and the latter may therefore be disposed to reject the theory *in toto*. But it is necessary here to distinguish two distinct questions: it is one thing to deduce the logical consequences of premises generally admitted, and another to inquire how far those premises are really applicable to the case in hand. With respect to the first point there can, I apprehend, be no doubt that the theory developed in the present paper stands on as high a level as that of Kirchhoff relating to a circular plate. It satisfies the same differential equation over the area, and the same boundary-conditions, and is indeed in one respect* even less open to doubt. But it may be that the fundamental equations are for some reason less applicable to a square than a circular plate, though I do not see why this should be the case. On the other hand it is possible that, in spite of all Dr. Strehlke's care, some error may have crept into his measurements. If this be so, the cause of error must be of a systematic character, not eliminated by repetition or by changing the plates. One suggestion I may be allowed to make: it appears to me that experimenters have not been sufficiently careful to touch the vibrating plates only at the nodes. In the present case it is, I believe, not unusual to damp the plate by touching the middle point of one of the sides—a method which would be legitimate on the old view that the node passes, or ought to pass, through the points referred to, but which certainly cannot be justified for purposes of measurement without an experimental or theoretical proof that no error is thereby introduced. One would suppose that the action of the finger or other damper must in part resemble the effect of a load applied at the point touched; and there can be no question that such a load would produce a disturbance.

XVII. *Notices respecting New Books.*

Astronomical and Meteorological Observations made during the year 1870, *at the United-States Naval Observatory.* Washington: 1873.

THIS volume, which commences with the report of the Superintendent of the Observatory, Rear-Admiral Sands, contains observations with the transit-circle, the meridian transit-instrument, the mural circle, and the equatoreal. From the report we learn that the equatoreal was under the charge of Professors Newcomb and Hall, who observed with it the minor planets, comets, and occultations. The transit-circle was under the charge of Professor Harkness, who also had charge of the telegraphic apparatus and connexions by which correct time is furnished to the Navy department, the city of Washington (by striking the fire bells

* The relation of the constants and rigidity and compressibility.

three times daily), and the principal railways in the Southern States. The transit-instrument and mural circle were under the charge of Professor Yarnall; and Professor Eastman had charge of the Meteorological Observations.

The attention of Professor Newcomb has been given chiefly to the theory of the moon, for the purpose of revising both the theory and the tables, in the course of which a new and more exact method of computing the effect of the attraction of the planets has been worked out. The results of the Professor's work indicate that the positions of the moon at the total eclipse of 1715 and at the epochs of two occultations of Aldebaran observed at Greenwich and London in 1680, are better represented by the old tables of Burckhardt than by those of Hansen.

The introductory part of the volume contains very full descriptions of each instrument, its various adjustments, method of observing adopted, determination of corrections, &c. These notices embody various important practical details which it is next to impossible to find in any popular treatise, but which are essential for the training of an efficient amateur astronomer. We need not enlarge on the high value of the observations recorded.

The volume contains four appendices. Appendix I. embodies the operations for determining the difference of longitude between Washington and St. Louis. The work was effected by the aid of the Western Union Telegraph Company in exchanging signals—the observations and reductions at St. Louis being made by the officers of the United-States Coast Survey, and those at Washington by the officers of the Observatory.

Appendix II., "Reports on Observations of Encke's Comet during its return in 1871," has already been noticed in our pages.

Appendix III., "On the Right Ascensions of the Equatoreal Fundamental Stars," by Professor Newcomb, contains much valuable information. The object the Professor had in view was to reduce the right ascensions of different catalogues to a mean homogeneous system. We may possibly return to this Appendix on some future occasion.

Appendix IV. contains the zones of stars observed at the Naval Observatory with the meridian transit-instrument in the years 1846 to 1849—a portion of the great work originally contemplated, but discontinued on account of the difficulty experienced by the computing staff in endeavouring to keep pace with the observing, a difficulty which threatens the extinction of our own system of meteorological observations.

We cannot close this notice without directing attention to the progress of Astronomy in America. Our own National Observatory, under the able management of the Astronomer Royal, continues to maintain its position as the most important astronomical establishment in the United Kingdom. There is, however, much useful work to be effected by amateurs, who may consult the volume before us with advantage.

XVIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.

[Continued from vol. xlv. p. 462.]

December 18, 1872.—Warington W. Smyth, Esq., F.R.S., Vice-President, in the Chair.

THE following communications were read:—

1. "Further Notes on the Punfield Section." By C. J. A. Meÿer, Esq., F.G.S.

This paper was supplementary to one read before the Society by the author in March of the present year (see Quart. Journ. Geol. Soc. xxviii. p. 245), and contained the results of a fresh examination of the section at Punfield, and of the Wealden and Neocomian strata of the Isle of Wight. He described the section exposed at his visit to Punfield as presenting:—1. True Wealden beds; 2. a grit-bed with limestone and paper-shales, containing fish-bones and Cyprides; 3. apparently argillaceous beds; 4. a thin band of hard ferruginous sandstone with Atherfield fossils; 5. a clay-bed, the upper part regarded as representing the "lobster-clay" of Atherfield, the lower sandy portion containing an abundance of marine fossils belonging to common Atherfield species; 6. the so-called "marine band;" and 7. laminated clays and sands with lignite. The author indicated the accordance of this arrangement with what is observed elsewhere, and maintained that the grit-bed (No. 2), with its limestone and paper-shales, containing *Cypris* and *Cyrena*, was really to be regarded as the passage-bed between the Wealden and the Neocomian.

2. "On the Coprolites of the Upper Greensand Formation, and on Flints." By W. Johnson Sollas, Esq.

The first part of this paper was principally occupied in an endeavour to explain the perfect fossilization of sponges and other soft-bodied animals. It was shown that the hypothesis which considered that sponges had become silicified by an attraction of their spicules for silica was altogether untenable. Mr. Hawkins Johnson's supposititious reaction, according to which the carbon of animal matter is directly replaced by silicon, was shown to be inconsistent with the known facts of chemistry. The author's explanation was not intended to be final. The first fact pointed out was the very remarkable way in which the silica or calcic phosphate of the fossils under consideration followed the former extension of organic matter. This was explained for silica by the fact that, when silicic acid is added to such animal matters as albumen or gelatin, it forms with them a definite chemical compound; and it was assumed that in process of time this highly complex organic substance would decompose, its organic constituents would be evolved, and its silica would remain behind. In such a way flints might be produced; and dialysis would lend its aid. The same explanation was applied to account for the connexion between calcic phosphate and animal matter in the case of the "Coprolites."

The Blackdown silicified shells were next explained; and it was reasoned that the state of their silica offered arguments tending to prove a passage of silica from the colloidal to the crystalline state.

The second part of the paper discussed the Coprolites specially. Their exterior appearance is extremely sponge-like, almost exactly resembling some species of modern sponges. They are marked by oscules of peculiar characters.

The so-called "pores" of palæontologists are well marked. Spicules, triradiate, sexradiate, sinuous, defensive and connecting, have been observed. They are siliceous in composition. On dissolving the coprolites in acid, the spicules are set free, associated with *Polycystina* (*Haliomma hexacantha* &c.) and *Xanthidia* (*X. furcatum*). The genera and species of coprolites described were as follows:—*Rhabdospongia communis, Bonneyia bacilliformis, B. cylindricus, B. Jessoni, B. scrobiculatus, B. verrongiformis, Acanthophora Hartogii, Polyacantha Etheridgii, Retia simplex, R. costata, Ulospongia patera, U. calyx, U. Brunii.* The external appearance of these forms, which constitute a vast number of the coprolites, their curious oscules and siliceous spicules, were said to leave no doubt as to their spongious origin.

XIX. *Intelligence and Miscellaneous Articles.*

A NEW METHOD FOR EXAMINING THE DIVISIONS OF GRADUATED CIRCLES. BY G. QUINCKE.

THE task of examining a graduated circle by which the position of two telescopes, with microscopes for reading off, was to be determined accurately to a few seconds, has led me to a method of examination combining convenience and accuracy; and, as far as I am aware, it has not yet been described.

The telescopes are provided with a Gauss's ocular (*Astron. Nachr.* 579. 31. 10, 1846), in which the cross-threads can be illuminated by a plane glass placed between them and the lense of the ocular and inclined at an angle of 45° to the axis of the telescope.

The axis of the telescope is placed normal to a plane-parallel glass plate, if the cross-wires coincide with the reflected image projected by the rays reflected from the plate.

The glass plate can be set up normal to the graduated circle by being fixed with wax upon a little turntable in its centre.

The axis of the telescope and its axis of rotation are exactly perpendicular to each other, if the cross-wires and their reflected image coincide also after the telescope has been turned through 180°. With different positions of the plane glass firmly united to the circle, we thus obtain also the excentricity of the axis of rotation (if any exist) in relation to the centre of the circle. For the sake of greater intensity of light I make use of Steinheil's plane-parallel glasses, one side of which is silvered and polished.

Two plane-parallel mirrors are fastened with wax upon the turntable, perpendicular to the axis of the telescope. They are exactly

perpendicular to each other, if the angular mirror which they constitute makes the two images (produced by double reflection) of the cross-wires of one of the telescopes coincide with the cross-wires themselves.

If the axes of the telescopes 1 and 2 are, through reflection of the illuminated cross-wires, set normal to the two faces of the mirror, they make exactly an angle of 90° with each other. Different positions of the angular mirror then determine every four points of the circle-division which are 90° distant from each other.

Two plane glasses make exactly an angle of 120° or 60° with one another, if two telescopes, placed each normal to one of them, show simultaneously, through double reflection, the cross-wires of telescope 1 in the cross-wires of telescope 2, and *vice versâ*.

By putting, with different positions of the angular mirror of 120° or 60°, the two telescopes normal to the respective faces of the mirror, we obtain, by reading-off the circle, points exactly 60° or 120° distant from one another.

If the two plane glasses make an angle of $180° - 2\phi$, and the telescope-axes, normal to them, an angle 2ϕ with each other, a third plane glass can be fastened with wax upon the turntable in the centre of the graduated circle in such manner as to reflect the rays proceeding from the cross-wires of telescope 1 upon the cross-wires of telescope 2. Plane glass 3 is then inclined at the angle ϕ to plane glass 1 or 2; and the angular mirror formed of 1 and 3 or 2 and 3 can be used to set the telescope-axes perpendicular to the mirror-faces, and to determine points in the circle whose distance from one another is measured by the angle ϕ.

From the angles 90° and 60° we thus obtain with this third plane mirror angles of 45° and 30°—and from these, angles of $22\frac{1}{2}°$ and 15°, &c.

Should there be any difficulty in bringing the plane glasses into correct position with wax and the free hand, it can be readily obviated by a simple arrangement with screw and pressure-spring.

The method of reflection of the cross-wires permits us also to compare the angles of glass prisms with those of the angular mirrors, and, with the unalterable angle of a glass prism of exactly 90°, 60°, 30°, 20°, 10°, &c., to measure and calibrate the divisions of the circle. The latter I have not been able to carry out, as the prisms long since ordered for the purpose are not yet in my possession.

The method above described is most convenient, and accurate to the extent of the magnifying-power of the telescopes and as far as the distinguishing of the ocular-threads is possible—that is, as nearly as one can in general see with the apparatus in question. As, also, the perfectness of the plane glasses can be easily controlled with the telescope, it is probably applicable to the production of a new and accurately graduated circle.

Würzburg, June 1, 1873.

ON A CONVENIENT EYEPIECE-MICROMETER FOR THE SPECTROSCOPE. BY PROFESSOR O. N. ROOD.

I have recently contrived a very simple eyepiece-micrometer for the study of spectra produced by prisms and "gratings," which, while quite inexpensive, is capable of yielding results that are not easily surpassed except by the use of an eyepiece provided with a micrometer-screw. A thin semicircular plate of silver is made quite smooth, and rendered black by holding it over the flame of a lamp; it is afterward flowed with a drop of weak spirit-varnish to cause the lampblack to adhere. Crossing the straight edge of this dead-black surface, lines $\frac{1}{4}$ millim. &c. are ruled with a dividing-engine, and the necessary figures added with the help of a lens. The opaque semicircular plate is then introduced into the interior of a negative, or preferably in front of a positive eyepiece, so that it is in focus and does not occupy quite half of the field of view. Opposite it and somewhat nearer the eye an opening is made in the side of the eyepiece, whereby the lines are brightly illuminated —as a general thing, merely by the diffused light of the room; but if this is quite dark, the small flame of a distant lamp easily accomplishes the same end. This arrangement, it will be seen, furnishes a set of bright lines on an almost perfectly black ground with the least possible outlay of expense or trouble in manipulation; and the *degree* of their brightness, it will be found, can readily be regulated merely by shading the opening more or less with the hand. The distance of the lines apart should not be too small, as is often the case in the photographic scales of ordinary spectroscopes, but such as will facilitate the estimation of tenths of a division. Two such eyepieces have been constructed and employed by me with much satisfaction in the mapping of a large number of spectra furnished by prisms and gratings, particularly in those cases where the spectral lines were quite faint.—Silliman's *American Journal*, July 1873.

JAMIN'S COMPOUND MAGNETS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Is not the method of making powerful compound magnets published by M. Jamin substantially identical with that employed by Scoresby, and fully set forth in his 'Magnetical Investigations' so long since as 1839? He shows that thin *hard* steel plates may be superposed on the other with great advantage if separated by thin slips of wood or cardboard, and states that the power of such plates accumulated efficiently "to the amount of 192, and might have been carried much further."

I am, &c.,
R. S. CULLEY.

General Post Office,
June 25, 1873.

Erratum in Number 304.'

In Dr. H. Morton's paper, fig. 6 (page 94) should be fig. 9, and fig. 9 (page 97) should be fig. 6, and should be transposed as under.

Fig. 6.

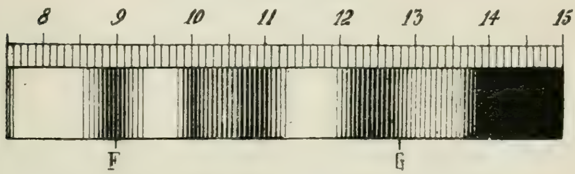
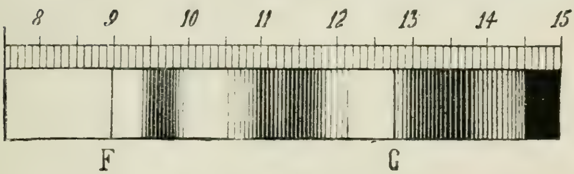


Fig. 9.



To the Binder.—**Please paste the above figures over those similarly numbered on pages 94 and 97, which, as they now stand, are transposed.**

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

SEPTEMBER 1873.

XX. *On the Effects of Magnetization in changing the Dimensions of Iron and Steel bars, and in increasing the Interior Capacity of Hollow Iron Cylinders.* By ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, New Jersey, U.S.A.*

[Continued from vol. xlv. p. 359.]

Part II. *On the Elongations and Retractions of Rods of Iron and Steel on their Magnetization and Demagnetization.*

TO study and measure with precision the minute elongations and retractions which rods of iron and steel undergo on their magnetization and demagnetization, it is necessary that the motions of the part of the measuring-apparatus which records these changes in length should not be in the least affected by outside vibrations transmitted to the apparatus, but should be controlled alone by the molecular motions in the rods which take place on changes in their magnetic condition; also the motions of this indicating part of the apparatus should be synchronous with the motions in the rods, so that we may be able to study the character as well as the amount of these elongations and retractions.

Several instruments have been devised by me which fulfil these essential conditions; but they were all abandoned (except one, to be described in detail in Part IV. of this memoir) and preference given to "the reflecting comparator and pyrometer" of our esteemed colleague Mr. Joseph Saxton. This simple and precise instrument is well known to American physicists as the apparatus which has greatly aided in giving to the geodetic work of our

national Coast Survey its renowned precision, and in rendering accurate the comparisons and constructions of our Office of Weights and Measures. A detailed description, with drawings, of this instrument will be found in the "Report on the Construction and Distribution of Weights and Measures, Washington, 1857," written by Dr. A. D. Bache, the late illustrious President of our National Academy*.

The measuring-instrument.

I will now describe the actual adaptation of this instrument to our research. The drawings (figs. 1 and 2) give respectively an elevation and a plan of the apparatus. A beam of Georgia pine, well seasoned, dried and then soaked with shellac varnish, formed the base on which the instrument was lined and firmly attached. This beam is 7 feet long, $8\frac{1}{4}$ inches deep, and $5\frac{3}{4}$ inches wide. It rested on slips of hard wood at *i*, *i*, placed at

Fig. 1.

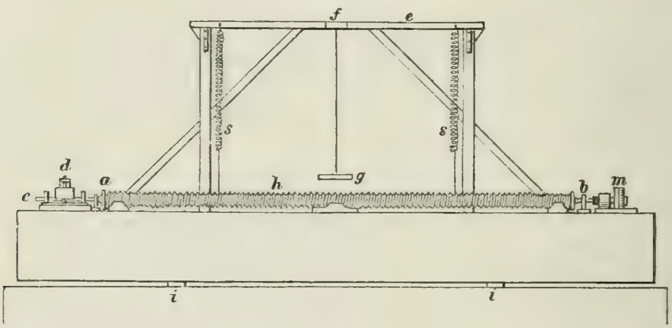
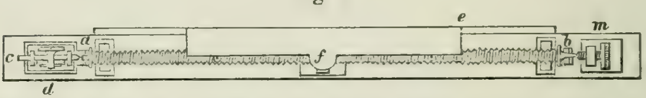


Fig. 2.

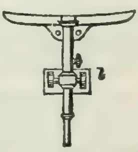


distances from the ends of the beam equal to one fourth of its length. At *a* and *b* are two Vs of brass which supported the terminal brass caps of the rods experimented on. These rods were all 60·1 inches long, ·5 inch in diameter; and each rod weighed on the average 1520 grammes. While the ends of a rod rested on the Vs, 1100 grms. of its weight was supported by the two springs *s*, *s*, which took hold of the rod at distances from its ends equal to one fourth of its length. The flexure of the rod was thus in great part avoided; and it could therefore

* To my friend and colleague Dr. J. E. Hilgard, of the United-States Coast Survey Office, I am indebted for the loan of the comparator used in these researches.

be accurately centred in the helix *h*. This helix is 60·25 inches long, and has an outside diameter of 1·75 inch. It is wrapped on a tube of brass of ·8 inch internal diameter, slit longitudinally throughout its whole length. At *m* is the micrometer abutting-screw, against which the end of the rod is firmly pressed by two heliacal springs, which are stretched between hooks on the brass mounting of the screw and a rod which passed through the terminal brass cap of the rod. These brass caps at both ends of the rod are terminated by pieces of agate. The other end of the rod is in contact with a slide *c*, with triangular section, which accurately moves, between guides, in a direction which is the axis of the rod prolonged. To this slide is attached a delicate fusee-chain, which is coiled once round the vertical axis of the mirror *d*. This chain is prevented from slipping by a steel pin, which securely attaches it to the axis. The slide carrying the chain is firmly pressed against the terminal agate of the rod by means of an heliacal brass spring. Thus the rod is at one end firmly pressed against the micrometer-screw, while against the other end presses the slide, which is connected with the mirror by the intervention of the fusee-chain, which latter is also tightly stretched. The well-joined framework *e* supports the springs *s*,*s*, and also a divided circle, *f*, from whose centre depended either a fibre of silk or a filament of glass, which supported a magnet of very hard steel, *g*. From the oscillations of this magnet, or from its deflections, by means of the divided circle and glass filament were determined the intensities of the residual magnetism of the rods. The deflections of the mirror, caused by the elongations or retractions of the rods, were determined by means of the telescope and scale represented at *l* (fig. 3). The telescope and scale were placed between 5 and 6 metres from the mirror; and the scale was divided into millimetres. Each unit of division given in the experiments corresponds to one centimetre of the scale.

Fig. 3.



The above apparatus was placed in a room without windows and entirely underground; and the room was always entered by descending from a door whose bottom was on a level with the ceiling of the room. During the course of the experiments the range of variation of temperature of this room did not exceed 0°·1 C. in twenty-four hours.

Examination of the stability and degree of precision of the apparatus.

A rod was accurately centred in the helix, and the micrometer-screw and the slide of the mirror were brought into contact with its ends. After the heat imparted to the rod and apparatus by

handling had been dissipated, and the scale-reading had remained constant for an hour, I pushed the mirror-slide away from the rod and then again allowed it to come against its agate terminal. This operation was performed as expeditiously as was consistent with the careful avoidance of shocks to the apparatus. The thermometers, which were placed near various parts of the apparatus, were now read; and it was ascertained that they gave the same readings as before the apparatus was touched. The scale was now read in the telescope; and it was ascertained that the cross-threads bisected the same division observed before the slide was moved, thus showing that the mirror returned to the position it had before its rotation. This important fact was repeatedly confirmed with all the rods and at various stages of the research. Indeed the measures of hundreds of experiments made on the elongations and retractions of rods also confirmed the confidence I obtained in the indications of the apparatus arrived at by the preceding experiments.

Jumping on the floor of the room and the passage of carriages and carts in the streets had not the slightest effect in disturbing the scale-readings.

To ascertain if the mirror accurately followed the changes in length of the rod, I repeatedly made the following observations. The readings of the screw-head and the scale were noted; then the screw was rotated by an assistant so as to push before it the rod. The scale-readings ran up steadily with the rotation; and when the screw was rotated backward the scale-readings smoothly ran down; and when the screw-head had reached the same position it had before it was touched, I found that the scale-reading corresponded to that noted when the screw previously had this position. This observation, repeatedly made, gave me the means of testing the precision of the instrument during the progress of the investigations.

It will be seen below that one division of the scale, or 1 centimetre, corresponded to a change of ·00011 inch in the length of the rod; but $\frac{1}{2}$ millimetre, or $\frac{1}{20}$ of a division, could be precisely read in the telescope; I am therefore justified in believing that the measures I shall give in this memoir can be relied on to the $\frac{1}{200000}$ of an inch.

Determination of the value of one division of the scale in changes of length, in inch-measure, of the rods.

Pasted on the inside of the box in which the mirror, telescope, and micrometer-screw came to me was the following:—"Abutting-screw of field pyrometer. By seven comparisons of five turns with 0·1 inch on Troughton-scale, in June 1857, by Mr. Saxton, 1 turn $=0{\cdot}01912$ inch."

As the result of numerous determinations made on various parts of the screw, I found that $\frac{1}{100}$ of a rotation of the screw equalled 1·737 division of the telescope-scale, which gives 0·00011 inch as the value of one division of the scale. This value, however, applies only to the experiments on the rods of iron Nos. 1 to 6 inclusive. Before commencing the experiments on the rods of steel, the distance of the scale from the mirror was changed; and in this new position I found that one division of the scale corresponded to 0·000146 of an inch.

Description of the helix, and measures of the resistances of its wires.

The helix was a compound one, formed of four layers of copper wire. The two inner layers formed 1069 turns of one length of 303 feet of wire ·087 inch in diameter. The two outer layers were formed of another length of 330 feet of ·112-inch wire wrapped in 850 turns. These two helices could be used separately, or joined into one helix of 633 feet having 1919 turns.

The resistance of the inner helix was ·44 ohm; the outer had a resistance of ·31 ohm, together giving a resistance of ·75 ohm. The latter resistance, added to that of the wires leading from the battery through a Gaugain galvanometer to the helix and back to the battery, brought up the resistance to nearly one ohm.

A *battery* of 25 cells of Bunsen was used in the determination of the coefficients of elongation and retraction; and the above interpolar resistance showed that the maximum effect of magnetization would be given by connecting the 25 cells, 5 in couple and 5 in series. Whenever in this research we speak of the effect of 25 cells, it is to be understood that they are connected as just described.

The iron and steel rods used in the experiments were prepared for me with the well-known skill and fidelity of Mr. Wallace, of Ansonia, Ct. He carefully selected the materials, and annealed the iron rods by packing them, with iron scales from a rolling-mill, in a wrought-iron covered box, and exposing the box to a red heat for three days; the box was then allowed to cool very slowly. The steel rods were tempered as uniformly as possible throughout their lengths.

Arrangement of the apparatus, and general description of the phenomena which take place on the magnetization and demagnetization of the rods of iron.

The beam supporting the apparatus was so placed that the axis of the helix was in the magnetic meridian. Each rod before it was introduced into the helix was tested as to its magnetic condition by placing its length at right angles to the

magnetic meridian and pointing it towards the centre of a magnetic needle. When the rod thus directed gave indications of polarity, its S. end was placed downwards, with the axis of the rod in the line of the dip, and its upper end was struck with a light mallet. The rod was tested until, after one or more operations of this kind, it gave no indications of polarity. But on placing the rod in the helix it, of course, was again magnetized, but feebly, by the earth's induction. This fact serves to determine the distance at which the magnet, which determined the residual polarity, had to be suspended above the rod. If this magnet is placed too near the rod, then an interaction between it and the soft iron of the rod takes place by the inductive action of the magnet, and the vibrations of the latter are more frequent than when it is alone acted on by the earth; but if it be removed to a certain distance above the rod, then the magnetism of the rod acts as a "damper" on the magnet, and its vibrations are slower than when it is only under the earth's influence. There is therefore an intermediate position, at which the magnet vibrates the same, whether the rod remains under it or is taken away. This distance, of course, varies with the rod used; but on the average it was about 3 inches.

Thus arranged the rod was allowed to remain until its temperature had become constant and the scale-reading in the telescope was stationary.

The interpolar connexions with the battery were made so that the helix, on closing the circuit, would magnetize the rod with the same direction of polarity it already had from the earth's action. The current was now passed from the 25 cells by plunging the amalgamated wire of the open part of the circuit into a cup of mercury; then the scale-reading was immediately noted; the circuit was at once broken and another reading obtained. The thermometers, which were placed on various parts of the apparatus, and which had been read just before closing the circuit, were now again observed and the room vacated and closed. At intervals of a half hour during the three to six subsequent hours the room was entered and readings of scale and thermometers obtained.

It may here be well to give a general account of the phenomena which the rods exhibit when the voltaic circuit is successively closed and opened. When the rod has for the first time the heliacal current passed round it, a sudden elongation takes place; and this elongation remains steadily of the same amount as long as the circuit is closed and the temperature of the rod remains constant. Now on breaking the circuit the rod retracts, with a less velocity than that with which it first elongated; but the retraction does not equal the elongation. The

temperature of the rod remaining constant, it has been found that the rod retains the length it attained on the breaking of the circuit; that is, the rod has received with its permanent magnetic charge a permanent elongation. On passing the current a second time, the rod again elongates; but the elongation is now less than that which took place when the current was first passed round it. On now breaking the circuit the rod retracts to the length it had before the current was passed for the second time; that is, after the first magnetization and demagnetization of the rod the successive elongations and retractions are equal. These conditions exist until four or five subsequent make- and break-circuits have been made: but now a change takes place in the phenomena; for on making the circuit the rod elongates about the same as in the preceding experiments, *but* on breaking the circuit the retraction does not equal the elongation; so that after each experiment the rod is slightly longer than before the preceding experiment was made. Continuing the experiments, the scale gradually passes over the cross-threads; and I have thus repeatedly caused the entire scale to traverse the field of the telescope. On now allowing the rod to remain at the temperature it had when the current was for the first time passed round it, the rod slowly retracts until, after several hours have elapsed, it has the length which was observed after the first experiment made upon it.

Heat developed in the rod at the instant of its demagnetization.

The above-described experiments show conclusively that the minute elongations which take place on breaking the circuit are due to the heat developed in the rod at the moment of its demagnetization; for in the preceding experiments the current did not heat the helix sufficiently to cause radiations from it to elongate the rod; therefore, to obtain the results described above, it is important to ascertain beforehand, when the current has traversed the helix for a time equal to that occupied in the experiments, that the rod during this time does not elongate.

If the current is sufficiently intense to heat directly the helix and rod, the above phenomenon of heating on demagnetization nevertheless manifests itself, and can readily be disentangled from the combined effects, as will be seen further on.

These interesting results, proving the development of heat on demagnetization, were obtained a year ago without any knowledge of the recent work of Jamin and Roger*; and these measures, made directly on the changes in length of the rods, tend to con-

* I have not been able to find the paper containing Jamin and Roger's experiments either in the *Comptes Rendus* or in the *Ann. de Chim. et de Phys.* I have obtained the information of their results only from the fol-

firm the result arrived at by those experimenters. Recently Cazin (*Comptes Rendus,* vol. lxxv. p. 1265) has shown that "the heat thus produced is proportional to the square of the intensity of the magnetism and to the polar distance;" and Moutier (*Comptes Rendus,* vol. lxxv. p. 1620)* deduces this result from a thermodynamic theorem established by Clausius, and thus concludes his paper:—"The increase of *vis viva* which the bar experiences from the effect of magnetization is therefore proportional to the square of the intensity of the magnetism and to the polar distance. The effect of the demagnetizing corresponds to an equal loss of *vis viva,* which is the measure of the thermal effect produced; and this effect is the only one which accompanies the demagnetization."

The fact that an iron bar is heated by successive magnetizations and demagnetizations has been known for a long time; but only recently have experiments been made which indicate that this heat is produced at the moment of demagnetization. In a paper "On the Calorific Effects of Magneto-electricity, and on the Mechanical Value of Heat" (Phil. Mag. S. 3. vol. xxiii. 1843), Dr. Joule first showed that heat was developed in an iron bar *when* it was rotated between the poles of a powerful magnet, and also determined that the heat thus produced was proportional to the square of the inductive force. These experiments will ever be regarded with interest; for they led Joule to the first experimental determination ever made of "the mechanical value of heat." It may here be of interest to present the following account of the experiments made by Van Breda and Grove, taken from Daguin's *Traité de Physique,* 1861, vol. iii. p. 621:—"M. Van Breda having enveloped a tube of iron with a helix through which he passed an intermittent current, found a heating of the iron, due to the alternative displacement of the molecules†, the heat being shown by the dilatation of the air contained in the tube, which formed the reservoir of an air-thermometer. Grove subsequently determined the heating of an armature of soft iron, on passing an intermittent current in the wire of an electro-magnet on which the armature was placed, or in turning near it

lowing passage in the paper of M. Cazin (*Comptes Rendus,* vol. lxxv. p. 1266):—"When we pass an intermittent current in the wire of an electromagnet, the recent experiments of MM. Jamin and Roger have demonstrated in a definite manner that the core is heated." The method by which they discovered this fact is not stated by Cazin. See *Ann. de Chim. et de Phys.* vol. xvii. (1869).

* Phil. Mag. Feb. 1873, p. 157.

† The heat observed, however, may not be entirely due to these motions; for the thermal effects may in part be due to the currents induced in the iron on magnetization and demagnetization.

the poles of a strong electromagnet. The heating effects were indicated by a thermo-electrical couple. Cobalt and nickel gave similar results, but less marked; whilst non-magnetic metals were not heated in the same circumstances." I have made many experiments on a tube of iron, weighing two hundred-weight, which confirm these results; the experiments will be given in Part III. of this memoir.

I here present two Tables of experiments on rod No. 2, of Ulster iron. The successive discussion of these two Tables will give to the reader a clear physical conception of the phenomena, and serve to elucidate the account I have above given of the heat developed on demagnetization.

Table I.

No. of experiment.	Scale, circuit open.	Scale, on closing circuit.	Scale, on breaking circuit.	Elongation.	Retraction.
1.	37·6	39·2	38·0	1·6	1·2
2.	38·0	39·2	38·0	1·2	1·2
3.	38·0	39·2	38·0	1·2	1·2
4.	38·0	39·2	38·0	1·2	1·2
5.	38·0	39·2	38·0	1·2	1·2
6.	38·0	39·2	38·1	1·2	1·1
7.	38·1	39·3	38·1	1·2	1·2
8.	38·2	39·4	38·3	1·2	1·1
9.	38·3	39·5	38·4	1·2	1·1
10.	38·4	39·55	38·6	1·15	0·95
11.	38·6	39·6	38·6	1·0	1·0
12.	38·6	40·0	38·85	1·4	1·15
13.	38·85	40·1	39·0	1·25	1·1
14.	39·0	40·2	39·1	1·2	1·1
15.	39·1	40·2	39·2	1·1	1·0
16.	39·2	40·3	39·2	1·1	1·1
17.	39·2	40·4	39·3	1·2	1·1

In exp. No. 1 we passed the current for the first time round the unmagnetized rod, and observed an elongation of 1·6 of a division of the telescope-scale; immediately after the observation we broke the circuit, which had remained closed about five seconds, and observed a retraction of 1·2 division; the rod now remained at a constant temperature for three hours, and the scale-reading remained steady at 38·0—thus showing that the rod had received a permanent elongation of ·4 of a division on receiving its charge of residual magnetism.

On repeating the experiment we find an elongation and retraction of 1·2 division, which is the quantity the rod retracted on the first break-circuit. Experiments 2 to 5 inclusive give the same result; but on the 6th and subsequent break-circuits we observe a retraction less than 1·2; and this effect we attribute

to the heat produced in the rod at the instant of its demagnetization. It is also noteworthy that, from the moment of breaking the circuit in an experiment until the forming of it in the succeeding one, the scale remained immovable. Taking 1·2 division as the amount of elongation and retraction due alone to magnetization and demagnetization, we can determine the mean amount of elongation at the moment of demagnetization as follows. The mean elongation in experiments 6 to 17 is 1·18 division. This is only ·02 of a division less than 1·2, and can candidly be attributed to the errors of observation; but the mean retraction of the same experiments is 1·08 division, which is ·12 of a division less than 1·2, and gives us the measure of the effect due to the heating of the rod at the moment of its demagnetization; for on keeping the rod at the temperature it had during experiments 1 to 5, we found that it gradually retracted until the scale again remained steady at 38·0.

Table II. is here given to show that nearly the same effects of elongation and retraction are observed when the rod is gradually elongating under the effects of heat radiated from the helix, when the latter has a powerful current passed through it.

Table II.

No. of experiment.	Scale, circuit open.	Scale, on closing circuit.	Scale, on breaking circuit.	Elongation	Retraction.
18.	51·4	52·8	51·8	1·4	1·0
19.	51·8	53·2	52·2	1·4	1·0
20.	52·2	53·4	52·4	1·2	1·0
21.	52·5	53·8	52·7	1·3	1·1
22.	52·8	54·0	52·9	1·2	1·1
23.	53·0	54·3	53·2	1·3	1·1
24.	53·2	54·5	53·5	1·3	1·0
25.	53·5	54·7	53·6	1·2	1·1
26.	53·8	55·2	54·2	1·4	1·0
27.	54·2	55·4	54·4	1·2	1·0
28.	54·4	55·6	54·5	1·2	1·1
29.	54·5	55·7	54·7	1·2	1·0
30.	54·8	55·8	54·75	1·0	1·05
31.	54·8	56·0	55·0	1·2	1·0
32.	55·0	56·2	55·2	1·2	1·0
33.	55·2	56·3	55·25	1·1	1·05
34.	55·25	56·4	55·4	1·15	1·0
35.	55·4	56·45	55·5	1·05	0·95

The experiments in the above Table were made on the same rod used in the experiments in Table I.; but before this new series was commenced I passed round the helix a stronger current than previously used, so that the rod was elongated by the heated helix from 39·2 divisions of the scale to 51·4 divisions; and while the scale was advancing to this reading, I determined

its rate of progress and found it to be 3·6 divisions in ten minutes. Therefore these experiments were made on the rod while it had a slow motion of elongation. The mean of the elongations is 1·22 division; the mean of the retractions is 1·04 division, which subtracted from 1·20 gives ·16 of a division for the effect of the heat on one demagnetization. The reduction of Table I. gave ·12 of a division for this effect. The difference in the two results I thus account for:—While the bar was slowly expanding from the heat radiated by the helix, the circuit was made and the elongation was immediately observed; but about five seconds elapsed before the reading could be obtained and the circuit broken; and during these five seconds the rod was expanding, but so slowly that its amount could not be read, but was often visible. That this minute expansion could not be determined was to be expected; for if the rod elongated from heat 3·6 divisions in ten minutes, it elongated only ·03 of a division in five seconds, and ·03 of a division was a quantity too small to be measured on the scale; but it nevertheless existed there, and during the continuance of 18 make-circuits would amount to $\cdot 03 \times 18 = \cdot 54$ of a division—quite an appreciable quantity when we come to calculate the mean with this fraction contained in the sum of the retractions given in the last column of the Table. Therefore, to obtain the effect of the heat developed at the moment of demagnetization, we should *subtract* ·03 from ·16, the heating effect of demagnetization determined without this correction. This gives $\cdot 16 - \cdot 03 = \cdot 13$ of a division, while from Table I. we deduced ·12 for the value of the same effect. The difference of only ·01 division in the two results is not, however, to be taken without some reserve; for in the calculations I assumed that the rod had the same rate of expansion under a closed circuit as under an intermittent one; and this I did because I have no means of determining the difference, if any exists.

Experiments similar to those just given were made on all the iron rods; and similar results were obtained.

Relations existing between the number of break-circuits, the heating of the rod, and its elongation.

At this stage of the investigation it became of interest to determine the above relation. For that purpose I drilled a hole 6 inches deep in the direction of the axis of rod No. 3, of Norway iron, and inserted into this hole a thermo-electric couple formed of two wires (one of copper, the other of iron). This compound wire was wrapped first with two layers of waxed silk, then with twelve layers of floss-silk; and over these layers I coiled two more layers of waxed floss-silk, leaving, however, the point of

junction of the wires exposed. This apparatus was introduced into the rod so that the uncovered point of the wire was about one millimetre from the bottom of the hole; and the space included between the point of the wire and the bottom of the hole was filled in some experiments with fine iron filings, in others with mercury. The terminals of the thermo-electric couple were connected with a delicate galvanometer. With the apparatus thus arranged I successively made 50, 100, 200, 300, and 400 break-circuits, taking care that the closed circuits preceding the break-circuits should all be of the same duration. After each series of break-circuits the elongation produced in the rod and the permanent deflection on the galvanometer-needles were noted; and the observations showed that the elongations and the increments of temperature in the rod were proportional to the number of break-circuits.

On the elongations and retractions observed in the iron rods as the strength of the magnetizing-current is gradually increased and diminished; and on the equality in the elongations produced by a definite current when it is gradually and when it is suddenly brought up to its maximum strength.

The observed sudden elongations taking place in an iron rod at the moment of its magnetization naturally led me to inquire if the quantity of this elongation was in any way due to the suddenness of the magnetizing action, and whether the elongation produced by a certain current which is gradually brought up to its maximum strength would equal that produced by the same current suddenly passed with the same maximum strength. This problem was also connected with a proposed simple and accurate means of measuring the changes in dimensions of bodies subjected to magnetization; and therefore I have examined it with care, in the following manner. I cut the thick copper wire leading from the battery to the helix, and firmly attached one of its loose ends to a support. Between this copper wire and the opposite wall I stretched a fine wire of German silver. The other loose end of the battery wire was bent into a sharp angle, and the vertex of this angle was well amalgamated. Now, by sliding this bent copper wire along the fine wire of German silver towards the other copper wire, I could gradually diminish the resistance; and on its touching the other end of the thick battery wire this interposed resistance vanished and the current gained its maximum strength. On slowly retracing our steps the resistance was gradually increased until the whole length of the fine wire was interposed; and then the resistance was at its maximum and the strength of the current at its minimum. But if we brought the two amalgamated ends of the copper wire into contact either with

or without the intervention of a mercury cup, we at once could suddenly send the current with its maximum intensity through the helix.

Mean results of first series of experiments. One cell in circuit. Resistance of fine wire $=\cdot 6$ ohm.

On gradually diminishing the resistance,

Fraction of length of fine interposed wire . .	1.	$\frac{3}{4}$.	$\frac{1}{2}$.	$\frac{1}{4}$.	$\frac{1}{8}$.	0.
Scale-readings .	went from 54·8 to 54·85	54·9	55	55·2	55·6	56·1

On gradually increasing the resistance,

Scale-readings	went from 55·5 to 54·8	55·6	55·8	55·9	55·95	56·1
Tangent-galvanometer .	$4\frac{3}{4}°$	..	..	.	..	$29\frac{1}{2}°$

Mean results of second series of experiments. One cell in circuit. Resistance of fine wire $=\cdot 9$ ohm.

On gradually diminishing the resistance,

Scale-readings	54·8	54·8	54·85	55	55·4	56·1

On gradually increasing the resistance,

Scale-readings	went from 55·25 to 54·8	55·4	55·65	55·8	55·9	56·1
Tangent-galvanometer . .	3°	..	..	..	..	$29\frac{1}{2}°$

Examining the results in the two series of experiments, we see that when the current was passed with all of the interposed resistance in the circuit, the scale went from 54·8 to 54·85, or moved ·05 of a division in the first series of experiments; but in the second series the current was too feeble to effect a measurable elongation, and it was not until one fourth of the fine wire was out of the circuit that the scale-readings began to increase. In both series of experiments the rapid increase in the rate of elongation is noticeable after three fourths of the fine wire was out of the circuit. After all the interposed resistance had been traversed and was out of the circuit, the elongation in both series of experiments amounted to 1·3 division of the scale. The same amount of elongation always occurred when the ends of the copper wires were brought together, or when the circuit was as suddenly formed by plunging the wires into a cup containing mercury. Therefore it is well established that a current of a definite strength will produce the same amount of elongation, whether that strength is suddenly or gradually attained. Indeed in some of the experiments over three minutes were oc-

cupied in gradually decreasing the interposed resistance until it was entirely out of the circuit; yet during this very slow increase of the current-strength the scale slowly and smoothly moved upwards in its readings, and when all the interposed resistance had been passed over the elongation again equalled 1·3 division.

The establishment of the above fact was of considerable importance; for it rendered applicable the following simple and precise method of measuring the change in dimensions of bodies on their magnetization. Two iron, steel, or bismuth bars are placed parallel to each other, in Vs, with their similar ends strongly pressed against a firm support, so that, if the rods change in length on magnetization, their free ends will move. Now imagine a lever so arranged that one end of it carries a plano-convex lens and the other end a micrometer-screw. The convex side of the lens is opposite a plane glass which terminates the end of one of the rods, while the point of the micrometer-screw touches the end of the other rod, against which it is pressed by a spring. A piece of plane glass, inclined, placed in front of the lens sends the light from a sodium flame down to the lens and plane glass behind it; and by means of a microscope we can look through the inclined glass on to the lens, and thus accurately view and measure the Newton's rings which we shall now observe. If round the rods we now pass a voltaic current of gradually increasing intensity, we shall see the rings gradually displaced; and from the amount and direction of this *displacement,* together with the knowledge of the wave-lengths of the rays of sodium-light, we can accurately determine the amount and direction of the motion of the ends of the rods. If, however, the current had passed at once with its full intensity, then would have followed a sudden displacement of the rings, but the amount and direction of this displacement it would have been impossible to determine. By making the arm of the lever which carries the convex lens longer than the arm which carries the screw, we can increase the delicacy of the apparatus; for it is understood that, as the rods move in the same direction, the rod carrying the plane glass moves towards the lens, while at the same time the other rod, through the intervention of the lever, pushes the lens towards the plane glass.

The examination of the experiments of the first and second series, contained under the heading "on gradually increasing the resistance," makes known a remarkable phenomenon. In these experiments the current with its maximum strength was first passed through the helix, and then it was gradually brought down to its minimum strength by sliding the copper battery-wire over the fine wire of German silver until the whole length of the latter was brought into the circuit. At the moment of

sending the current with its maximum strength, the rod elongated 1·3 division of the scale; but if we now keep the circuit closed but gradually diminish the strength of the current, we observe that the scale-readings do not correspond to those given when the corresponding strengths of current were reached by going from their minimum to their maximum, as the following Tables, giving the differences of scale-readings in the two cases, clearly show.

First Series of Experiments.

Fraction of length of fine interpolar wire. . .	1.	$\frac{3}{4}$.	$\frac{1}{2}$.	$\frac{1}{4}$.	$\frac{1}{8}$.	0.
On gradually diminishing the current. .	55·5	55·6	55·8	55·9	55·95	56·1
On gradually increasing the current . .	54·85	54·9	55·0	55·2	55·6	56·1
Differences . . .	·65	·7	·8	·7	·35	0

Second Series of Experiments.

Fraction of length of fine interpolar wire . .	1.	$\frac{3}{4}$.	$\frac{1}{2}$.	$\frac{1}{4}$.	$\frac{1}{8}$.	0.
On gradually diminishing the current .	55·25	55·4	55·65	55·8	55·9	56·1
On gradually increasing the current . .	54·8	54·8	54·85	55·0	55·4	56·1
Differences . . .	·45	·6	·8	·8	·5	0

We thus see that the rod tends to persist in the elongation it acquired on first passing the maximum current; for it does not retract in proportion to the diminished strength of this current; and the experiments show that even when the current is so far diminished in strength that it would, if suddenly thrown through the helix, be unable to elongate the rod sufficiently to be measurable, yet this feeble current holds the rod elongated ·45 of a division in the second series of experiments; but on breaking the circuit the rod instantly retracts ·45 of a division in the second series of experiments and ·65 of a division in the first series, and regains the length it had before the current was passed round it.

On passing the current with the whole of the fine wire in the circuit, we have in the first series of experiments an elongation of ·05 of a division; but on making the circuit without the interposed fine wire we have an elongation of 1·3 division; and if we now do not break the circuit, but gradually diminish its strength by increasing the interpolar resistance, we find that, when the whole of the fine wire is again in the circuit, the

elongation is yet ·65 of a division, whereas when the circuit is at once formed with this same interposed resistance the rod was elongated only ·05 of a division.

The discovery of this most remarkable phenomenon was contained in the above experiments; but to be sure that my experiments should not mislead me I repeated them several times, using every precaution to ensure their accuracy, and obtained results almost identical with those formerly observed. I am therefore confident that I have discovered a phenomenon worthy of minute study; and I purpose to make it the subject of a special investigation.

Unfortunately, during the above experiments I did not make a parallel series of determinations of the magnetic intensities of the rod during the successive stages of passing a current of increasing and of decreasing strength. Yet I can hardly believe that the magnetic intensity will be kept up with the persistent elongation of the rod when it is slowly demagnetized; I think it will be found that the magnetic intensity of the rod depends alone on the strength of the current traversing the helix. The phenomenon indeed shows that, the molecules of the rod on its elongation by magnetization having been forced into new positions, the molecules, either by what might be well called *a "magnetic set"** or from molecular friction, retain these new positions with such persistence that it requires the sudden shock of the induced current produced on breaking the circuit, to cause them to rush to their positions of stable equilibrium.

Effects observed on making and breaking separate currents in the two component helices of the compound helix.

In these experiments two batteries were used. In the outer helix I made and broke a current from sixteen cells, arranged four coupled and four in series. In connexion with the inner helix I used a battery of 25 cells, connected five in a row and five in series. The experiments are interesting, as showing the effects of the induced currents formed on making and breaking the circuits in the various manners given in the following:—

(1) Made	circuit	in inner	helix;	rod elongated	1·4	division.
,,	,,	outer	,,	,,	·25	,,
Broke	,,	,,	,,	retracted	·25	,,
,,	,,	inner	,,	,,	1·4	,,

* The term "magnetic set," as applied above, is, by analogy, an appropriate name for the phenomenon; but it cannot well be so applied, because Dr. Joule has already appropriated "magnetic set" as designating the residual magnetism an iron rod retains after its electromagnetization.

(2)	Made	circuit in	outer	helix;	rod elongated 1·5 division.
	,,	,,	inner	,,	rod suddenly retracted ·4 and then suddenly elongated ·4.
	Broke	,,	,,	,,	rod suddenly retracted ·4 and then suddenly elongated ·4.
	,,	,,	outer	,,	rod retracted 1·5 division.
(3)	Made	circuit in	inner	helix;	rod elongated 1·4 division.
	,,	,,	outer	,,	,, ,, ·25 ,,
	Broke	,,	inner	,,	rod suddenly retracted ·35, then suddenly elongated ·35.
	,,	,,	outer		rod retracted 1·65.
(4)	Made	circuit in	outer	helix;	rod elongated 1·5 division.
	,,	,,	inner	,,	rod suddenly retracted ·5, then suddenly elongated ·5.
	Broke	,,	outer	,,	rod retracted ·1 division.
	,,	,,	inner	,,	,, 1·4 ,,

On the times occupied in the elongations and retractions of a rod when the two component helices are joined as one helix and placed in the circuit of one battery.

The determinations I here give were made with the eye and a chronograph, and although not as accurate as the interest of the research demands, yet are near enough to the truth to show that the subject is worthy of a careful investigation. The experiments given under the above heading and the succeeding one give an insight into the *velocities* of the molecular motions; and therefore these determinations, taken in connexion with the measures of the corresponding elongations and retractions, will be of considerable theoretic interest when they have been determined with the precision which the following proposed apparatus will in all probability afford.

I thus propose to attack this problem. The mirror of the apparatus will be made of the minimum weight consistent with stability. The mirror will reflect a pencil of light from an electric lamp to a revolving glass disk coated with sensitized collodion. This converging pencil will form a dot of light on the disk, and when the latter is stationary will, on the elongation of the rod, describe a portion of one of its radii, which will appear on developing the sensitized plate. If, however, the disk have a uniform and known rate of rotation, the dot will, on the elongation of the rod, describe a curved line, which, referred to the appropriate ordinates, will give not only the time of the motion of elongation, but also the mode or law of this motion. Of course the motion of retraction can be studied in like manner.

The following experiments were made on rod No. 3, of English refined iron; and each result is the mean of fifty experiments.

	Time of elongation.	Time of retraction.
(1) 25 cells. . . .	$\frac{1}{20}$ of a second.	$\frac{3}{10}$ of a second.
(2) 1 cell	$\frac{3}{10}$,,	$\frac{1}{10}$,,

It is thus seen that with twenty-five cells the duration of retraction is six times as long as the duration of the elongation; but with a current from one cell the phenomena are *reversed*, and the duration of the elongation is three times that of the retraction.

Determinations of the times occupied in the elongation and the retraction of a rod when the inner or the outer helix forms in itself a closed circuit, while the current is passed in the respective cases in the outer and in the inner helix.

(1) Terminals of inner helix *not* joined. Current passed through the outer helix from twenty-five cells. Elongation of the rod 1·5 division. Time of elongation $\frac{1}{120}$ of a second. Time of retraction $\frac{1}{4}$ of a second.

(2) Same results as above when the outer helix was open and the current was passed through the inner helix.

(3) The terminals of inner helix united, so that this helix formed a closed circuit in itself. Current from twenty-five cells passed through outer helix. Elongation 1·5 division.

Time of elongation $\frac{2}{10}$ of a second. *Time of retraction* $1\frac{1}{10}$ of a second.

(4) Same results as above when the terminals of the outer helix were united and the current passed through the inner helix.

(5) One cell used. When the terminals of outer or inner helix were *not* united, and the current passed respectively through inner or outer helix, the elongation was 1·1 division, the time of elongation $\frac{3}{10}$ of a second, the time of retraction $\frac{2}{10}$ of a second.

(6) One cell used. The terminals of inner helix united. The elongation was 1·1 division. Time of elongation $\frac{6}{10}$ of a second. *Time of retraction* $1\frac{3}{10}$ of a second.

(7) Same results as experiment (6) when the terminals of outer helix were joined and the current from one cell passed through the inner helix.

To observe a rod slowly retracting during 1·3 of a second was a most remarkable sight, and suggests many thoughts as to the interaction of the induced currents passing in the helices and rod. I may here venture to suggest that the study of these extraordinary phenomena (which I believe I have here first made known) will eventually be of some service in the investigation of

induced currents. For the present I am content with merely presenting *the facts*; for I have not yet been able to command the time which their investigation will require.

In experiments (3) and (6) the times of retraction were respectively $1\frac{1}{10}$ of a second and $1\frac{3}{10}$ of a second; and the slowness of these motions allowed me to obtain an insight into their *character*. In each of these experiments the rod retracted with a gradually diminishing velocity, and the motion reminded one forcibly of that pertaining to a body projected vertically upwards.

The coefficients of elongation and of retraction of seven rods of different species of iron, and of three steel rods of various degrees of hardness.

It remains to give the determinations I have made of the coefficients of elongation and of retraction. These measures were made on rods of circular section, 60·1 inches long and ·5 inch in diameter. As previously stated, the iron rods were thoroughly annealed, and the steel rods were carefully tempered. On the ends of the rods numbers were stamped; and these marks corresponded to the rods as follows:—

1 . . .	Scrap iron.
2 . . .	Ulster iron.
3 . . .	Norway iron.
4 . . .	English refined iron.
5 . . .	Low-Moor iron.
6 . . .	Fall-River iron.
000 . . .	Steel, soft.
00 . . .	„ hardened and drawn to blue.
0 . . .	„ „ „ yellow.

The method of determining these coefficients was as follows:—When the rod had attained a fixed temperature, so that the scale-reading remained constant for an hour, I recorded this scale-reading. I then passed the current from the 25-cell battery; and as soon as the new scale-reading thus produced was read, I broke the circuit and obtained the corresponding scale-reading. These readings were now written in the note-book; and immediately after recording them I again made and broke the circuit, and noted the two corresponding readings of the telescope-scale. I then continued making and breaking the circuit and recording the scale-divisions until the rod began to elongate from the heat produced on demagnetization.

The Tables following (see p. 200) consist of six columns, A, B, C, D, E, and F. Under A are designated the rods. B contains the elongations or retractions produced on first passing the current; C the retractions or elongations observed after the first-

made circuit had been broken; D the permanent elongations or retractions observed in the rod after the first current passed had been broken; E the elongations or retractions produced on making the second and subsequent circuits; F the elongations or retractions produced on breaking the second and subsequently formed circuits.

After the quantities given in the columns I have written *e* to designate the *elongation* of the rod, and *r* to indicate its *retraction*.

I have given the measures in three Tables. Table I. contains the elongations and retractions in the actual scale units. It is here to be remembered that one division of the scale equals 0·00011 of an inch for the experiments on rods Nos. 1 to 6 inclusive; while for the remaining rods, 000, 00, and 0, one division of the scale equals 0·000146 of an inch. Table II. gives the elongations and retractions of Table I. expressed in fractions of the inch of "Troughton's scale"*. Table III. contains the coefficients calculated from the numbers given in Table II.

Certain numbers in the Tables are followed by * or by †; * indicates the maximum effect observed in the iron or in the steel rods corresponding to the phase of experiment given in the heading of the column in Table I., or as subsequently designated by A, B, C, &c.

An examination of the Tables shows that the maxima and minima effects in the case of the iron rods are very irregularly distributed. Thus, corresponding to the "first make-circuit," we find that rod No. 4 gives the maximum, while rod No. 1 the minimum. On the "first break-circuit" rod No. 2 is the maximum, while rod No. 5 is the minimum. For the "permanent elongation" rod No. 4 is the maximnm, and rods Nos. 1 and 2 are the minima. In the two columns corresponding to "second make-circuit" and "second break-circuit," we see that rod No. 3 gives the maximum effect observed, while rod No. 1 gives the minimum.

* "Two copies of the new British standard, viz. a bronze standard, No. 11, and a malleable-iron standard, No. 57, have been presented by the British Government to the United States. A series of careful comparisons (made in 1856 by Mr. Saxton, under the direction of Dr. Bache) of the British bronze standard No. 11, with the Troughton scale of 82 inches, showed that *the British bronze standard yard is shorter than the American yard by* 0·00087 *inch*. So that, in very exact measures with the yard-unit, it is necessary to state whether the standard is of England or of the United States, as 10,000 American feet = 10,000·5803 English feet."—Lecture-Notes on Physics, by A. M. Mayer, p. 12 (Van Nostrand, New York, 1868).

The phenomena of elongation and retraction observed in rods of steel.

The phenomena observed on the magnetization and demagnetization of the rods of steel have not been referred to. Here we have presented to us remarkable results. On first passing the current round rod 000, of soft steel, it elongated ·8 of a scale-division, behaving like a rod of soft iron; *but* on breaking the circuit, to my astonishment it again *elongated* ·6 of a division, thus leaving this rod with a *permanent* elongation of 1·4 of a division; and this elongation exceeds the permanent elongation given to any of the soft-iron rods when similarly experimented on. On passing the current round the rod *for the second time* the soft-steel rod again did not act like a rod of iron; for it *retracted* ·25 of a division, instead of *elongating* as did the rods of iron in like circumstances; and on breaking this circuit the rod *elongated* ·25 of a division instead of *retracting*, again exhibiting a phenomenon the reverse of those observed in the rods of iron. And it is here important to remark that *all* the steel rods behaved in the same manner on the making and breaking of the second and subsequently formed circuits.

The results just described differ from those obtained by Dr. Joule. Referring to his memoir (Phil. Mag. vol. xxx. p. 85), we find that experiments on a rod of soft steel, 1 yard long and $\frac{1}{4}$ of an inch in diameter, showed that the rod elongated on first passing the current; but on breaking this circuit the rod *retracted*, while in my experiments the rod again *elongated* on breaking this circuit. Indeed the experiments of Dr. Joule indicate that a rod of soft steel behaves like one of iron, except that the elongations and retractions are of less extent than in the case of an iron rod. It is important, however, to observe that Dr. Joule did not, in his first experiment on this rod, pass round it a current sufficient to "saturate" it, but gradually increased the intensity of the current in successive experiments; and it is to be remarked that, as the intensity of the current increased, the retractions and elongations came nearer and nearer to equality; but in no instance did he observe a *retraction* on passing a current and an *elongation* on its cessation.

In his subsequent experiments Dr. Joule worked on a steel rod of the same dimensions as that used in his former experiment; but it was "hardened to a certain extent throughout its whole length, but not to such a degree as entirely to resist the action of the file." On *first* passing the current, and also on subsequently passing the current with successively increased intensities, he obtained results similar to those I observed in the rod of soft steel; but with this rod also he never observed a

retraction on making a circuit and an *elongation* on breaking it. The fact that so eminent an investigator as Dr. Joule obtained, on *first* passing a current round a bar of hard steel, results similar to those obtained by me with my bar of soft steel, leads me to suspect that the rod I experimented on may have retained some degree of "hardness" after it had been annealed; but even this fact granted does not explain why *all* the steel rods I experimented on gave *retractions* on passing the current a *second* time after they had been "saturated" during the *first* passage of the current.

Examining the results of my experiments on rod 00 (of hard steel "drawn to blue"), we see that the phenomena are exactly the reverse of those occurring in rods of iron in the same circumstances, except in this one particular, viz. that after breaking the first-made circuit the rod is permanently *elongated*; and this result agrees with all of those obtained by Dr. Joule.

The experiments on rod 0 (of hard steel "drawn to yellow") are noteworthy. On making the *first* circuit this rod *retracted* ·4 of a division; and on breaking this circuit the rod elongated, but only ·25 of a scale-division, thus leaving the rod permanently *retracted* ·15 of a division; so that this rod of hard steel, which after the experiment remained a powerful magnet, is *shorter* than it was before it had been magnetized. On passing the second current round this rod, it, like the two preceding steel rods, *retracted* ·2 of a division; and on breaking this circuit it elongated by the same quantity; so that after the second and subsequent passages of the voltaic current it persisted in the retraction it received after the first-made circuit was broken.

The experiments I have just given on rod 0 differ in every particular from any obtained by Dr. Joule on rods which *were not subjected to tractile strain.* I cannot but regret that this eminent physicist did not experiment on rods of very hard steel freed as far as possible from all strains; for then my experiments would have been strictly comparable with his. The experiments which Dr. Joule made on rods of hard steel (except those I have already quoted) were conducted on rods subjected to tractions going from 80 lbs. up to 1030 lbs., while my experiments were made on rods so supported by brass springs that only a fraction of their weights was supported by the Vs on which their ends rested.

Referring to Dr. Joule's experiments on a "soft steel wire 1 foot long, $\frac{1}{4}$ of an inch in diameter, tension 80 lbs.," we find that this rod behaved like one of soft iron free from strain with currents deflecting his galvanometer from 34° 40′ up to 56° 30′; but with currents below 34° 40′ no action whatever was observed to take place in the rod, except its magnetization; but when the

same rod was subjected to a tension of 462 lbs. and a current of 60° 15′, it behaved like my horizontally suspended steel rod 00; that is, it retracted on making the circuit, but it elongated more than it had previously retracted when this circuit was broken. With a tension of 1680 lbs. the rod retracted and elongated by equal amounts on making and breaking the circuits. In Dr. Joule's experiments on a "hardened steel wire, 1 foot long, $\frac{1}{4}$ of an inch in diameter, tension 80 lbs.," he observed no effects until the current reached an intensity of 45° 40′; then this rod also elongated and retracted by equal quantities on making and breaking the circuits. With tensions of 408 lbs. and of 1030 lbs. this rod behaved in the same manner, but the elongations and retractions did not begin to show themselves with the respective tensions until the currents had respectively reached the intensities of 60° 20′ and 48° 33′. Summing up these results, Dr. Joule states:—"From the above experiments we find that the induction of permanent magnetism produces no sensible effect on the length of a bar of perfectly hardened steel, and that the temporary shortening effect of the coil is proportional to the magnetism multiplied by the current traversing the coil. The shortening effect does not in this case sensibly increase with the increase of tension." We have no reason to doubt the truth of this statement when applied to rods subjected to tension; but my experiments show that when the rod 000 (of soft steel) and the rod 00 (of hard steel "drawn to blue") were not subject to such strains, and indeed freed as far as possible from all strain, they were *permanently elongated* after they had received their permanent magnetism, and also that the rod 0 (of hard steel "drawn to yellow") had a *permanent retraction* given to it with its permanent magnetic charge.

My experiments have been made with such conscientiousness that at present I am not able to doubt the reality of these effects; but they should be repeated on fresh bars, and this I intend to do at some future day.

It is important that I should here call attention to the fact that the coefficients I have given in the appended Tables are derived from measures on only *one* rod of each species of metal; and it may be that a considerable range in the elongations and retractions may be found in rods made of the same kind of iron or of steel. I hope to be able to present a new series of determinations of these constants, to be made with the apparatus already described, which employs the displacement of Newton's rings as a means of measuring the changes in the longitudinal and transverse dimensions of the rods.

When it is considered that the greatest motions which have been the objects of my study have their existence in the space

of $\frac{3}{10000}$ of an inch, while the smallest pass within the limits of visibility of the most powerful microscope, being only $\frac{1}{200000}$ (or ·000005) of an inch, and, furthermore, when it is known that the last-mentioned quantity equals the change in length of one of the rods caused by a variation of temperature of only $\frac{7}{1000}$ of a degree Centigrade, the difficulties I have conscientiously met and surmounted in this delicate research become manifest; but the very knowledge of these difficulties has tempered with modesty the confidence I feel in my work, and I will gladly accept any correction my measurements may receive from more experienced hands.

Tables of the Elongations and Retractions of the Rods.

TABLE I.—Elongations and Retractions in Units of the Telescope-scale.

A. Rod.	B. 1st make-circuit.	C. 1st break-circuit.	D. Permanent *e* or *r*.	E 2nd make-circuit.	F. 2nd break-circuit.
1	1·25 *e*†	·75 *r*	·4 *e*†	·7 *e*†	·7 *r*†
2	1·6 *e*	1·2 *r**	·4 *e*†	1·2 *e*	1·2 *r*
3	2·0 *e*	·9 *r*	1·1 *e*	1·4 *e**	1·4 *r**
4	2·5 *e**	1·15 *r*	1·35 *e**	1·15 *e*	1·15 *r*.
5	1·65 *e*	·6 *r*†	1·05 *e*	1·0 *e*	1·0 *r*
6	1·4 *e*	·85 *r*	·55 *e*	·9 *e*	·9 *r*
000	·8 *e**	·6 *e**	1·4 *e**	·25 *r*	·25 *e*
00	·25 *r*	·5 *e*	·25 *e*	·5 *r**	·5 *e**
0	·4 *r*†	·25 *e*†	·15 *r*†	·2 *r*†	·2 *e*†

TABLE II.—Elongations and Retractions in fractions of an inch.

A.	B.	C.	D.	E.	F.
	inch.	inch.	inch.	inch.	inch.
1	·0001375	·0000825	·000044	·000077	·000077
2	·000176	·000132	·000044	·000132	·000132
3	·000220	·000099	·000121	·000154	·000154
4	·000275	·0001265	·0001485	·000126	·0001265
5	·000181	·000066	·0001155	·000110	·000110
6	·000154	·0000935	·0000605	·000099	·000099
000	·0001168	·0000876	·0002044	·0000365	·0000365
00	·0000365	·0000730	·0000365	·0000730	·0000730
0	·0000584	·0000365	·0000219	·0000292	·0000292

* Maximum. † Minimum.

TABLE III.—Coefficients of Elongations and of Retractions.

A.	B.	C.	D.	E.	F.
1	·000002288 *e*†	·000001377 *r*	·000000732 *e*†	·000001281 *e*†	·000001281 *r*†
2	·000002928 *e*	·000002196 *r**	·000000732 *e*†	·000002196 *e*	·000002196 *r*
3	·000003660 *e*	·000001647 *r*	·000002013 *e*	·000002562 *e**	·000002562 *r**
4	·000004575 *e**	·000002105 *r*	·000002471 *e**	·000002088 *e*	·000002088 *r*
5	·000003019 *e*	·000001098 *r*†	·000001921 *e*	000001830 *e*	·000001830 *r*
6	·000002562 *e*	·000001555 *r*	·000001006 *e*	·000001647 *e*	·000001647 *r*
....	....	....	....	....	....
000	·000001943 *e**	·000001457 *e**	·000003400 *e**	·000000607 *r*	·000000607 *e*
00	·000000607 *r*	·000001212 *e*	·000000607 *e*	·000001212 *r**	·000001212 *e**
0	·000000972 *r*†	·000000607 *e*†	·000000364 *r*†	·000000486 *r*	·000000486 *e*†

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XXI. *An Inquiry into the Nature of Galvanic Resistance, together with a theoretic Deduction of* Ohm's *Law and the Formula for the Heat developed by a Galvanic Current. By* E. EDLUND ‡.

1. IT is known from the science of light that the æther in material substances has a greater density than in empty space. Matter must therefore possess the power of attracting the molecules of æther, while these repel each other. Matter condenses within itself æther from the surrounding space until the attraction between the molecules of the matter and an æther molecule outside the substance is no greater than the repulsion between the æther already condensed by the body and the said exterior molecule of æther. Since the resultant of these two forces upon the external free æther is $=0$, the body together with the æther condensed by it exert no influence upon the equilibrium of the free æther, but this distributes itself just as if the body and the condensed æther were in reality not present. It follows that, if there is to be equilibrium in the mass of æther, the free æther within the body must have the same density as that outside of it. If, for example, the free æther within were for a moment to possess less density than the external æther, equilibrium would inevitably be restored by æther streaming into the pores of the body; and if the relation were reversed, æther must stream out. The density of the free æther in all material bodies is therefore equal. Consequently the æther within bodies consists of two portions: one is attached by the attraction of the molecules of the body, and may be greater or

* Maximum. † Minimum.

‡ Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlviii. p. 419, having been read before the Royal Academy of Sciences at Stockholm on the 11th September, 1872.

less, according to the nature of the body; the other portion is free, and its density equal in all bodies. Of course, however, this does not prevent the æther from experiencing resistance in its passage from the one place to the other. This conclusion was partially verified by Fizeau's well-known investigations on the passage of light through a fluid in motion; for he was led by his investigation to this result—that one portion of the æther adheres to the molecules of the fluid, while the other portion must be considered free and independent of that motion*.

According to our view, the galvanic current consists of nothing but the transport of the free æther in the direction of the length of the conductor; and, in a previous memoir†, we have endeavoured to adduce proofs for the correctness of this view. The quantity of æther which the circuit contained when the æther was still at rest, is neither augmented nor diminished by the formation of a current; it is merely put into translatory motion by the electromotive forces. In ordinary galvanic currents these forces expend heat in producing this motion; so that cooling must ensue at the place where they are in action—perhaps in the same way as a gas compressed within a vessel is cooled when it gets an opportunity of issuing through an opening, in which operation heat-vibrations are expended in order to effect a translatory motion of the gas-particles. The electromotive forces act directly upon the adjacent strata of æther only, and set them in motion; and, through the pressure hence arising, these occasion the motion of the rest of the mass of æther. As is well known, the æther has very great elasticity. It may therefore be assumed that the pressure producing this motion cannot very much alter the density of the moving æther.

According to the theory we present of electrical phenomena, the distribution of the electroscopic tensions upon the surface of the conductor which unites the poles of the electromotor is an immediate effect of the current itself. In the hitherto received electrical theory, on the contrary, the electroscopic distribution has been taken for a basis, from which it has been attempted to deduce both the dependence of the current-intensity on the electromotive force and on the resistance (Ohm's law), and also the law of the development of heat by the current. But since, according to the æther theory, the electroscopic distribution is an effect of the current, and a phenomenon has not to be deduced from its effects, but from its causes, we have held it expedient to theoretically establish these two laws independently of the electroscopic distribution.

* *Comptes Rendus*, vol. xxxiii. p. 349. Pogg. *Ann.* Ergb. iii. p. 457.

† *Archives des Sciences Phys. et Nat. de Genève*, 1872. Pogg. *Ann.* Ergb. vi. Hefte 1 & 2.

2. We will first endeavour to ascertain what we are to understand by the expression "galvanic resistance."

We picture to ourselves a tube, the cross section of one half of which is I, and that of the other half nI, filled with a fluid which is in translatory motion in consequence of forces acting upon it at one end of the tube. If, now, at one place in the tube we wish to lessen the motion or its velocity by a counterpressure (*e. g.* a piston or the like), we must apply n times as much pressure in the wider half as in the narrower, in order to produce the same effect. The diminution of the velocity or the strength of the current does not depend on the absolute quantity of this counterpressure, but on its quantity on the unit of surface of the cross section. If the pressure on the unit of surface is equally great in the wider and in the narrower tube, the lessening of the strength of the current is the same in both cases. This will be the relation, whatever may be the nature and constitution of the resistance; only the particles of the fluid must be sufficiently mobile to propagate the pressure in all directions. What has just been said is directly applicable to a galvanic current. Whatever view one may entertain on the nature of electricity, all are perhaps agreed in this—that it is a fluid the particles of which are readily movable, and that it must therefore possess the property of communicating pressure in all directions. Galvanic resistance obstructs electric motion. It thus produces a counterpressure; and this is equally distributed over all points of the cross section of the conductor. When, for example, two wires of different metals and of unequal thickness produce an equal diminution of a given current-intensity, these resistances are said to be equal; and we have then, in accordance with the foregoing, to assume that, on the unit of surface of the cross section, the counterpressure which each of them opposes to the propagation of the current is also equal. Consequently it is only the counterpressure on the unit of the cross section that can come into question in the determination of resistances. This is a consequence of hydrodynamical laws, and cannot be conceived in any other way, inasmuch as electricity is a fluid.

That galvanic resistance depends on the physical and chemical constitution of the conductor is readily understood; but the possibility can also be foreseen that it may be dependent on other conditions also. The resistance might be regarded as arising from the friction experienced by the æther molecules in pressing through between the material molecules of the conductor. We have already remarked that the density of the free æther in all bodies is equal. Therefore in equal volumes there are equal quantities of free æther. If, then, a current comes from a wire with the cross section I, and passes into another

wire in which the section is n times as much, there come in the thicker wire, at every cross section, n times as many molecules of æther into motion; for it is inconceivable that in the thicker wire any more æther remains at rest than the inconsiderable portion which appears as electroscopic tension. Yet, because the strength of the current is the same in both wires, the velocity in the thicker one must be only one nth part of that in the thinner wire. Then each molecule of æther in the thinner wire goes in the unit of time n times as far as those in the thicker. We cannot, therefore, hold it an impossibility that the resistance is greater in the former case than in the latter, because the resistance may be dependent on the velocity. How it is with it in reality is decided by experiment, which teaches that the resistance is inversely proportional to the transverse section of the conductor.

We will imagine a single conducting wire f, of cross section I, and also other wires f', f'', f''', &c. of the same material, cross section, and length as the first mentioned, laid close beside one another, and that one and the same current S runs through the wire f and then simultaneously through the n laterally combined wires f', f'', f''', &c. Then through each of the latter a current of the strength $\frac{S}{n}$ must pass. But we know from experiment that the resistance which the current has to overcome in order to pass simultaneously through f', f'', f''', &c. amounts to $\frac{1}{n}$ of the resistance which must be overcome when the current passes through f. According to the foregoing representation, the counterpressure on the unit of surface of the cross section in the n wires must also amount to one nth of that in the single wire f, because the resistance is determined exclusively by the quantity of the counterpressure on the unit of surface of the cross section. From this it follows that in each of the n wires f', f'', f''', &c. the resistance must amount to $\frac{1}{n}$ of that of the single wire f. Now in each of these n wires the intensity of the current is $\frac{1}{n}$ of what it is in the wire f. We thus arrive at the unexpected result that the resistance is proportional to the intensity.

This result contradicts the, until now, universally received view, according to which the resistance should be independent of the intensity. But if any one will maintain this view, he must also, for the reasons before given, assume that for the fluid which we call electricity the laws of motion are altogether different from those which hold for other fluids with which we are

acquainted. Moreover it will subsequently be shown that, although the proposition here advanced is contrary to the general view, it is in no way inconsistent with the experimental evidences on which it has been supposed that view could be founded.

Conformably to experiment and the above theoretical considerations, we obtain the following as the expression for the resistance r in a conductor of length 1 and cross section a when the current s passes through it:—

$$r=k\frac{s}{a}=r_0 s,$$

where k is a constant dependent on the temperature and on the physical and chemical constitution of the conductor. The constant k is evidently the resistance of a conductor whose cross section is 1, and its length 1, when it is passed through by a current of intensity 1.

$\frac{s}{a}$ is the current-intensity on the unit of surface of the cross section; r_0, or what has been hitherto named the galvanic resistance, is simply the resistance for the unit of intensity of the current.

3. We now imagine a closed circuit whose length is l, and its cross section everywhere a, and which consists throughout of the same material and is passed through by a constant current with the intensity s. If δ is the mass of æther in motion in the unit of volume, and h its velocity, $s=a\delta h$. To calculate the mechanical work performed by this current in the unit of time, let us first consider separately a current-element whose length is 1. Because the counterpressure on the unit of surface of the cross section is r, and the magnitude of the section a, the counterpressure on the whole of the cross section becomes $ra=ks$. In the unit of time this element is moved forward the distance h; wherefore the work done will be ksh. Now h is $=\frac{s}{a\delta}$, in which, as was shown above, δ is constant. The mechanical work, for this element, thus becomes $\frac{ks^2}{a\delta}$. Multiplying this expression by l, we get the work of the whole current, $=\frac{kls^2}{a\delta}$. When, finally, this is multiplied by the heat-equivalent A of the unit of work, and the constant δ is combined with k, we obtain the quantity of heat produced by the current in the unit of time, $=\frac{\mathrm{A}kls^2}{a}$, a result which, as is known, agrees with experiment.

According to the same principles the calculation can be easily

effected also for the case in which the cross section and the constitution are different at different parts of the circuit.

4. Ohm's law can be deduced, in accordance with the general principles of mechanics, in the following manner:—Electromotive force is measured, just like other motive forces, by the acceleration which it can impart to the unit of mass in the unit of time. If no galvanic resistance existed to obstruct the motion, the velocity would increase perpetually. But there is a resistance in the conduction, which sets a limit to this increase. If the velocity has become constant, the acceleration by the electromotive force is annihilated by the resistance. The two must therefore be equal. If ds is the increment of the current-intensity in the time dt, E the electromotive (accelerating) force, ms the total resistance with the current-intensity s, and L the length of the entire conduction, we have

$$L\frac{ds}{dt}=E-ms*.$$

If, now, the current has become constant (that is, $ds=0$), then

$$s=\frac{E}{m}.$$

The deduction we have given of Ohm's law shows that it does not hold before the current has become constant. When the preceding equation is integrated and the time from the first commencement of the current calculated, we obtain the following formula for the increment of the current—

$$s=\frac{E}{m}\left(1-e^{-\frac{mt}{\alpha}}\right).$$

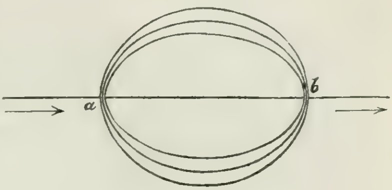
Herein no account is taken of extra currents; so that the formula holds only on the hypothesis that the path of the current is so constituted that no currents of that sort arise on its being closed. The formula shows that the less the length of the current-path, and the greater the resistance for the intensity 1, the more quickly does the current become constant, but that the electromotive force has no influence on the time required for this.

5. We will now adduce some applications of the formula

* Because the total length of the conduction L is equal to the sum of all its parts $(l_1+l_2+l_3+\ldots)$, its total volume is $a_1l_1+a_2l_2+a_3l_3+\ldots$, if those parts have respectively the cross sections a_1, a_2, a_3, &c.; and multiplying this sum by δ, we obtain the total mass of æther which is in motion. If now the increments of the velocity in the time dt are expressed by dh_1, dh_2, dh_3, &c. respectively, the total mass of æther receives in the time dt an increment of its quantity of motion which is expressed by $\delta(a_1l_1dh_1+a_2l_2dh_2+a_3l_3dh_3+\ldots)$. But $\delta a_1dh_1=\delta a_2dh_2=\delta a_3dh_3=ds$, by which, therefore, the increment of the total quantity of motion becomes equal to Lds.

found for the galvanic resistance. A galvanic current s divides itself (see fig. 1), at a point a on the conduction-path, between several similar conductors $f_0, f_{\prime}, f_{\prime\prime}$, &c., which with the intensity 1 have the respective resistances r_0, $r_{\prime}$, $r_{\prime\prime}$, &c., and all meet in a point b. We have now to determine the quantity of the current that passes through each of these conductors.

Fig. 1.



It is obvious that the current s must so distribute itself that the resistances in all the conductors shall be perfectly equal—that is, that the resistance undergone by each of these portions of the current during its passage from the point a to the point b shall be equally great. If the resistance in one of the conductors were for a moment less than in the rest, the intensity there would augment till the resistance became as great as in the rest. Naming the respective currents s_0, $s_{\prime}$, $s_{\prime\prime}$, &c., we obtain the following, because the resistance is proportional to the intensity:—

$$s_0 r_0 = s_{\prime} r_{\prime} = s_{\prime\prime} r_{\prime\prime} = \ldots = \mathrm{R}.$$

This signifies that the current-intensities in the respective conductors are inversely proportional to the resistances with the current-intensity 1,—a result which, as is well known, accords with experiment.

The conductors $f_0, f_{\prime}, f_{\prime\prime}$, &c. are now to be replaced by a single conductor F, so that this alone shall cause the same resistance as the conductors $f_0, f_{\prime}, f_{\prime\prime}$, &c. together.

The resistance is determined by the counterpressure, against the propagation of the current, on the unit of surface of the cross section of the conductor. This pressure was, when the current passed simultaneously through $f_0, f_{\prime}, f_{\prime\prime}$, &c., $=$ R. If the resistance sought (that of the conductor F for the current-intensity 1) is denoted by x, we have

$$\mathrm{R} = sx.$$

In order to find R, we have, according to what was stated above,

$$s_0 = \frac{\mathrm{R}}{r_0}, \quad s_{\prime} = \frac{\mathrm{R}}{r_{\prime}}, \quad s_{\prime\prime} = \frac{\mathrm{R}}{r_{\prime\prime}},$$

and therefore

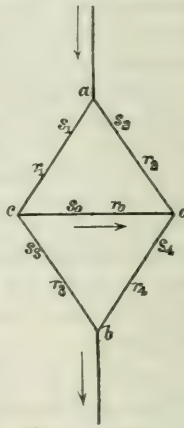
$$s_0 + s_{\prime} + s_{\prime\prime} + \ldots = s = \mathrm{R}\left(\frac{1}{r_0} + \frac{1}{r_{\prime}} + \frac{1}{r_{\prime\prime}} + \ldots\right).$$

From this we obtain

$$x=\frac{1}{\frac{1}{r_0}+\frac{1}{r_I}+\frac{1}{r_{II}}+\dots},$$

which, as is known, agrees with experiment. Let us now imagine such an arrangement of the conducting wires as is shown in fig. 2. The conductor divides at a into two branches, which again unite at b; and the branches are connected by the bridge $c\,d$. At the point c the current divides into two portions, one of which passes through $c\,b$, and the other through $c\,d\,b$. Then, according to what has just been adduced in reference to the equality of the resistances in the two conductors, we must have the following expression:—

Fig. 2.



$$s_3r_3=s_0r_0+s_4r_4.$$

In like manner the current divides at a into two parts. Then the resistance in $a\,d$ must be just as great as in $a\,c$ and $c\,d$ together; for if the resistance in $a\,d$ for example were less, the current-intensity in this conductor must grow until the resistance became just as great as in the other two conductors together, so that the current would have the same resistance to overcome in order to arrive at d from a, whether it passed through $a\,d$ or through $a\,c\,d$.

We thus get

$$s_1r_1+s_0r_0=s_2r_2.$$

If we will that no current traverse the bridge $c\,d$, therefore that s_0r_0 shall be $=0$, the ratio which for this purpose must subsist between the resistances is obtained by dividing the first formula by the last; while it is to be remembered that in this case $s_1=s_3$ and $s_2=s_4$. In this way we get

$$\frac{r_1}{r_3}=\frac{r_2}{r_4}.$$

All these formulæ are among the oldest known.

More examples are not requisite, as those above given are sufficient to prove the applicability of the expression for the galvanic resistance.

6. It has been remarked above, that, if galvanic resistance is independent of the intensity of the current, it must be admitted that the electric fluid follows other hydrodynamic laws than those

followed by the rest of the known fluids. Another contradiction arising from the assumption that the resistance is independent of the intensity is the following:—The electromotive force is operative during the whole time the current continues. If, then, the conductor placed no obstacle to the motion occasioned by that force, the velocity must, according to ordinary mechanical laws, unceasingly rise (that is, the intensity of the current must continually increase) as long as the electromotive force was acting. If, on the other hand, the conductor opposes to the motion a resistance independent of the intensity of the current, two cases may occur: namely, the resistance is as great or greater than the acceleration produced by the electromotive force; or it is less than this. But it is clear that in the former case no current can commence, and that in the latter the current must continually increase in intensity. To preserve the current-intensity constant is, under this assumption, and applying the usual mechanical principles, impossible. If, however, contrary to all analogy with the rest of material substances, we deny *vis inertiæ* to the electric fluid, this contradiction can, it is true, be discharged. An accelerative force acting on a material mass which is destitute of *vis inertiæ* (that is to say, which instantly comes to rest as soon as the force ceases to act), cannot give this mass an accelerated motion. As Clausius remarks*, in the deduction of Ohm's law it has in reality been tacitly assumed that an electric mass has no *vis inertiæ*, or else that this is so little that no account need be taken of it. People have thus been able to maintain the constancy of the current-intensity, although it was assumed that the resistance is independent of it. But surely we have no right to attribute to the electric fluid qualities which contradict the universal nature of matter. We will, in conclusion, direct attention to the following relation:— A galvanic current divides itself between two conductors in the inverse ratio of their resistances. If, then, as has been hitherto assumed, the resistance were actually constant, and were in the one conductor greater, but in the other less, than the accelerating force, the current must pass exclusively through the latter. This would be a case exactly like the following:—Through a tube with a certain cross section, which divides into two branches of the same cross section, a fluid is urged by a pressure applied at the extremity of the main tube. If a counterpressure be applied in one of the branches greater, and in the other less than the pressure mentioned, the fluid must flow exclusively through the latter branch. Here, therefore, the assumption that galvanic resistance is independent of the intensity of the current appears to be contradicted by experiment. It is quite different if the

* Pogg. *Ann.* vol. lxxxvii. p. 424.

resistance is proportional to the intensity: as we perceive from what has been before adduced, in that case the division must take place in the way experiment teaches that it does.

But if galvanic resistance actually is, as we have endeavoured to demonstrate, proportional to the intensity, it may possibly be asked how this circumstance could so long have escaped observation in the determination of the resistances of conductors. The reason for it can readily be discerned. When the resistance of a conductor is to be investigated, either the conductor is inserted in the undivided circuit of a galvanic series, and the diminution of intensity thereby produced is compared with the diminution of the same current-intensity occasioned by another conductor of known resistance, or else a division of the current is made use of by employing a differential galvanometer or a Wheatstone's bridge. In the former case Ohm's formula is used for the calculation, and in the latter the formulæ which give the division of the current among several conductors. But in all these formulæ no other resistances occur than those belonging to the unit of intensity. Therefore, in the methods employed, only resistances with equal intensity, namely intensity 1, have to be compared one with another; and from such a comparison it is impossible to draw the conclusion that the resistance increases with the intensity. Consequently the question of the dependence of the resistance on the intensity can only be solved in a theoretical way.

XXII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* *By* James D. Dana.—Part II. *The Condition of the Earth's Interior, and the connexion of the facts with Mountain-making.*—Part III. *Metamorphism**.

[Continued from p. 140.]

II. *The Condition of the Earth's Interior.*

THE condition of the earth's interior is not among the geological results of contraction from cooling; but these results offer an argument of great weight respecting the earth's interior condition, and make it desirable that the subject should be discussed in this connexion. Moreover the facts throw additional light on the preceding topic—the origin of mountains.

It seems now to be demonstrated by astronomical and physical arguments (arguments that are independent, it should be noted, of direct geological observation), that the interior of our globe is essentially solid. But the great oscillations of the earth's sur-

* For Part I., on the Origin of Mountains, see p. 41.

face, which have seemed to demand for explanation a liquid interior, still remain facts, and present apparently a greater difficulty than ever to the geologist. Professor LeConte's views (Am. Journ. Sc. vol. iv.) were offered by him as a method of meeting this difficulty; yet (as he admits in his concluding remarks) the oscillations over the interior of a continent, and the fact of the greater movements on the borders of the larger ocean, were left by him unexplained. Yet these oscillations are not more real than the changes of level or greater oscillations which occurred along the sea-border, where mountains were the final result; and this being a demonstrated truth, no less than the general solidity of the earth's interior, the question comes up, how are the two truths compatible?

The geological argument on the subject (the only one within our present purpose) has often been presented. But it derives new force and gives clearer revelations when the facts are viewed in the light of the principles that have been explained in the preceding part of this memoir.

The Appalachian subsidence in the Alleghany region of 35,000 to 40,000 feet, going on through all the Palæozoic era, was due, as has been shown, to an actual sinking of the earth's crust through lateral pressure, and not to local contraction in the strata themselves or the terrains underneath. But such a subsidence is not possible, unless seven miles (that is, seven miles in maximum depth, and over a hundred in total breadth) of *something* were removed in its progress from the region beneath. If this *something* was only vapour or gas, then seven miles of open space must have existed there; and this could not have been, except through seven miles of local contraction along the region; but such an open space, if of possible formation, would have been obliterated by catastrophic subsidence, instead of the slow movement that actually took place. And moreover, such open spaces, of no less extent, must have existed, in one or more ranges, underneath all continental borders. This is proved, and at the same time the extreme improbability of their existence demonstrated, by the facts reviewed beyond.

If the matter beneath was not aërial, then liquid or viscous rock was pushed aside. This being a fact, it would follow that there existed, underneath a crust of unascertained thickness, a sea or lake of mobile (viscous or plastic) rock as large as the sinking region, and also that this great viscous sea continued in existence through the whole period of subsidence, or, in the case of the Alleghany region, through all Palæozoic time—an era estimated on a previous page to cover at least thirty-five millions of years, if time since the Silurian age began embraced fifty millions of years.

The under-Appalachian fire-sea, if a reality, must hence have had a long continuance.

But on the above condition it could not have begun its existence later than the period of disturbance closing pre-Silurian time. Earlier great subsidences were involved in the deposition of the material of which the Blue Ridge, Highland Ridge, Adirondacks, and the Archæan heights further north were made; and the undercrust sea would have been through all a necessity. In fact it is difficult to find a reason for doubting its having dated back to the era of general fluidity.

Directly following the Palæozoic, or as its closing event, as explained on a preceding page, occurred the plicating of the Alleghany rocks to their depths miles below, and the crystallization of part of them; and this epoch ended in the making of the mountains (a synclinorium) and the annexation of the central and western part of the region to the essentially stable area of the continent; and if motion in the rocks was ever transformed into heat, the under-Appalachian sea should have had its temperature, or its extent, or both, increased. Then, after the Appalachian region had thus become essentially stable, the locus of the region of yielding was moved to the eastward. The long range of the Triassico-Jurassic beds, on the Atlantic border from Nova Scotia to southern North Carolina, show the positions of the new troughs, as stated at page 133. These subsidences, amounting to 4000 feet in some parts, ended in a tilting of the beds and in fissure eruptions through all these sandstone regions from the most northern to the most southern. Now the question arises whether the great under-Appalachian fire-sea of the Palæozoic continued on through the Triassic and Jurassic periods of the Mesozoic, and thus favoured the subsidences and eruptions that then took place—or whether the old sea of viscous rock, after being increased in extent or temperature by the profound plicating and faulting of the Appalachian revolution, then ceased to exist (in some way difficult to understand), and others were made further east by the later movements. Such a ceasing with a subsequent renewal is seemingly improbable; and if it did not occur, then the under-Appalachian fire-sea continued from the Palæozoic far into the Mesozoic era.

When the material of the under-Appalachian sea was pushed aside by the subsiding Palæozoic deposits of the Appalachian region, what became of it? Some of it may have moved off southward. The chief part would pass either to the *west* or to the *east*. That it did not go *west* is evident from the ascertained fact that the oscillations in that direction during Palæozoic time were small; for the region was then the larger part of the time a mediterranean salt-water basin or sea, nur-

turing crinoids, corals, and mollusks, and making limestones. If not westward, then it passed *eastward*; and if driven eastward, a geanticlinal elevation of a sea-border region parallel with the area of subsidence must have been in progress from lateral pressure. The height of this geanticlinal, or swell of the overlying crust (anticlinorium), would depend on the distance to which escape eastward was possible—that is, on the eastward extent of the subterranean region of mobile rock. Its elevation was probably small and of varying extent during the Silurian and Devonian; for Devonian fossils show that the sea-border south of New York had some way an open connexion with the Atlantic Ocean; but there is no evidence in the Appalachian rocks of the Carboniferous era to prove that off New Jersey it was not at that time almost or quite a complete barrier: the marine fossils in the more eastern of the Pennsylvania Coal-measures are rare; and those in the western Pennsylvania beds would be from the waters of the Mediterranean Sea over the Mississippi basin, which reached northward from Alabama and, east of the Cincinnati uplift, bathed all the western part of the Appalachian region, and probably its whole breadth.

When, at the beginning of the making of the Alleghanies, the strata commenced to yield before the pressure and to become pushed up into great folds, the geanticlinal of the sea-border would subside in part in consequence of the removal of resistance in front of it; and this tendency to subside by gravity may have been part of the means by which the plication was made to go forward, its action adding to that of the pressure. But the subsidence did not continue to the obliteration of the geanticlinal; for it was still above the ocean's level during the following era—the Triassico-Jurassic. The absence of all remains of distinctively marine fossils from these rocks, and from any rocks of the Triassic and Jurassic eras in view over the Atlantic border, demonstrate (as I have long held) that an emerged area then existed outside of the present coast-line. Moreover, inasmuch as these Triassico-Jurassic areas (situated on the Atlantic slope parallel with the Appalachians) were subsiding while their rocks were in progress, the sea-border anticlinorium should at the time have taken another turn upward as a counterpart to this subsidence.

With the close of the Triassico-Jurassic era, if not before, the great *anticlinorian* barrier began actually to disappear; and by the time the Cretaceous period opened it had so far sunk that the Atlantic coast-region south of New York was again exposed to the ocean and flourished with abundant marine life, the Cretaceous fossils of the coast giving full evidence on this point. Thus the absence from the present Atlantic border of all Triassic

and Jurassic marine fossils and the presence of Cretaceous species in great numbers are well accounted for.

Professor Hunt has recognized the existence, on the Atlantic border of the continent and outside of it, during the Palæozoic era and earlier, of an emerged region, and has appealed to various bare Archæan areas in New England and to the north-east, and to the Archæan character of the Blue Ridge, &c., as proof. He has designated the region, badly, as an "eastern continent," and finds in it, with reason, a source for much of the sedimentary material that was used in making the Appalachian and other rocks. Professor LeConte also brings into his views such an elevation, and remarks upon its final disappearance. But neither of these authors states that he regards it as part of a system of oscillations set in motion by the lateral pressure resulting from the earth's contraction, and a direct counterpart to the geosynclinal of the Appalachian region. Their views are adverse to such an idea, the subsidence with them being not due to contraction.

The facts thus sustain the statement that lateral pressure produced not only the subsidence of the Appalachian region through the Palæozoic, but also contemporaneously, and as its essential prerequisite, the rising of a sea-border elevation, or geanticlinal, parallel with it, and that both movements demanded the existence beneath of a great sea of mobile rock.

The movement and mountain-making over other parts of the Atlantic border (p. 132), and also the grand double series of events on the Pacific or Rocky-Mountain border (p. 133), sustain and illustrate the same views. The undercrust fire-sea on the Pacific border must have had great length from north-west to south-east—and also great breadth, for the border region is at least 1000 miles wide; and great breadth and great length seem plainly its characteristics even till Tertiary times. And did it continue on through the Tertiary and afford the floods of rock that were poured out from the deep fissures of this long era? And was it still in existence when the great floods were poured forth over the drift-gravel beds?

It is further to be noted that, in the course of past time, the whole continent has had its surface from one side to the other criss-crossed with oscillations and lines of disturbance, from the lateral pressure acting against its opposite sides, whence it is clear that the continental subterranean seas were *once* continuous. An appeal to the other continents for further testimony is hardly necessary.

The facts from the ocean seem to demand a vastly greater range for the undercrust mobile layer. The coral-island subsidence during the Quaternary and part or all of the Tertiary

could hardly have been due simply to radial contraction from cooling; for this would make the cooling over the tropical part of the ocean in this small part of geological time sufficient to produce there a sixth of the oceanic depression. Is it not proof that even then the plastic layer had enough of extent beneath the tropical part of the oceanic crust to permit of such a sinking under the irresistible lateral pressure at work? However this may be, it stands as a fact to be explained.

In view of the conclusions here reached with regard to the earth's interior, I present the following statements:—

1. That this restriction of the interior liquidity of the earth to an undercrust layer does not require in itself any modification of the views I presented more than twenty-five years since on the results of the earth's contraction, since there is still a flexible crust and mobile rock beneath it.

2. The condition of the earth's interior here recognized is, as many readers will have observed, that suggested long ago by Professor W. Hopkins, the author who first offered a mathematical argument in favour of the earth's either having a very thick crust or being solid throughout*. In a paper "On Theories of Elevation and Earthquakes" in 1847†, Professor Hopkins argues that the central mass of the earth became solid in consequence of the pressure whenever the temperature within reached a limit that permitted of it, that crusting at the surface from cooling commenced afterwards, and that between the regions of interior and exterior solidification there long remained a viscous layer, which in the progress of time was gradually contracted by the union of the solid nucleus to the thickening shell.

3. The possibility of solidification at centre from pressure in the face of a temperature too high for consolidation from cooling has not been experimentally demonstrated; yet a number of facts favour the principle. It has been urged that since the solidification of rocks is attended by contraction (that is, by increase of density), and since pressure tends to produce this

* Trans. Roy. Soc. 1839, 1840, 1842.

† Report Brit. Assoc. for 1847, p. 33. The theory of elevation advocated in this paper attributes elevation, not to lateral pressure from contraction, but to evolved vapours underneath the elevated region. The array of facts which have been presented respecting the positions of mountain-ranges, their relations to the great areas of depression, their successive formation on sea-borders in parallel ranges, and the natural evolution of the whole from the universal action of the one great cause (contraction), has appeared to me to afford the most complete demonstration that the vapour theory is not necessary, at least as regards mountain-ranges. The fact also that mountains so raised could not hold themselves up has seemed to be an insuperable difficulty to the success of the method.

greater density, therefore pressure may bring about the condition of the solid. The fact that ice, which has less density than water, changes to water under pressure, has been appealed to in support of the conclusion. The pressure to which the material within the earth is subjected is so great that experiment can never imitate it or directly test its effects. Beneath only one hundred and fifty miles of liquid rock it would be not less than one million of pounds to the square inch. Less than this may have been sufficient to produce crystallization, and so give rigidity to the viscous rock-material, or, at least, after the cooling the earth has undergone. The rigidity of slowly solidified rock is beyond that of glass or steel, or the degree which, according to Sir William Thomson, must exist in order that the earth should be as completely free as it is from tidal movements in its mass.

4. According to the above, the solid part of the globe consists, as regards origin, of three parts:—

(*a*) The central mass, consolidated by pressure; the solidification *centrifugal*, or from the centre outward.

(*b*) The crust proper consolidated by cooling; the solidification *centripetal*, or from the surface inwards.

(*c*) The outer crust or superficial coatings (the supercrust), made chiefly by the working over and elaborating of the material of the surface through external agencies, aided by the ever-acting lateral force from contraction, and including all terrains from the Archæan upward.

5. As to the thickness of the viscous layer and the overlying crust, or the depth of the later undercrust seas, I have nothing to offer. The Appalachian subsidence might have been accomplished with but seven or eight miles of depth underneath.

The undercrust fire-seas would have their heat from time to time supplemented through the movements of the crust. But the ordinary oscillations of the crust were so extremely slow and so ineffectual in producing heat, and the greater mountain-making movements occurred at so very long intervals (many millions of years), and then were so very limited in area compared with the earth's surface, that this cause could not have prevented a gradual narrowing of their limits with the progressing refrigeration. But even after the general union of the crust and nucleus, giving the earth *trap-like* "rigidity," had taken place, leaving only local fire-seas, the connexion may not have been so complete that it would not sometimes yield enough to the slow working of lateral pressure to permit oscillations of nearly continental extent, like those of the post-Tertiary.

A final word on Mountain-making.—From the above we learn that in the work of mountain-making in eastern North America there was first the commencing and progressing geanticlinal on the sea-border, and, as a concomitant effect of the lateral pressure, a parallel geosynclinal further west along the border of the continent. Concurrently the deepening trough of the geosynclinal was kept filled to the water-level, or nearly, by sedimentary accumulatious until these had become seven miles in thickness; and, as a consequence, the lines of equal temperature (isogeotherms) in the crust beneath gradually rose upward seven miles; and further, the geosynclinal crust, owing to this rising of heat from below, lost part of its thickness by a melting-off of an under portion, and also part of its strength up to a higher level by the softening action of the heat, while it received, as the only compensation for the loss of thickness, the addition of half-consolidated sediments above. Finally, the geosynclinal region, owing to its position against the more stable continental mass beyond it, and to the weakness produced in its crust in the manner explained, became, under the continued lateral pressure and the gravity of the geanticlinal, a scene of catastrophe and mountain-making after the manner described.

The principle here brought in, that the weakening of the crust through the rise of the isogeotherms was one occasion of the catastrophe, is made of prominent importance by Professor LeConte (Am. Journ. Sci. vol. iv. p. 468), though by a somewhat different method.

The geological facts thus far gathered have not yet proved that there was a geanticlinal on the Pacific border (like that of the Atlantic), as a counterpart to the geosynclinals in progress; but the evidence may be looked for with confidence.

III. *Metamorphism.*

The fact that all metamorphic or crystalline rocks are upturned or plicated rocks has led many to believe that disturbance and plications were essential features of an epoch of metamorphism, and that Herschel's theory, which attributes metamorphism to the heat that rises into the strata owing to accumulation above (a rise of the isogeotherms), is insufficient. This conclusion is certainly confirmed by finding no evidence of metamorphism in the lowest beds of the Carboniferous series of Nova Scotia, where, since the series has a thickness of nearly 16,000 feet, according to two of the best geologists in the world, Logan and Dawson, the bottom temperature must have been, when the series was completed, at least 330° F. It is still better sustained

by the observation that the lower of the forty thousand feet of strata in the Appalachian region were, where measured by the Professors Rogers, *not metamorphic*, the Chazy and Trenton limestones being ordinary uncrystalline limestones. And yet the temperature in these inferior beds, marked by the ascending isogeotherms, must have been before their disturbance, as calculated by Professor LeConte, not less than 800° F., and much above this if more heat escaped from the earth then than now. Thus seven miles of accumulations were not sufficient to bring about metamorphism or crystallization even in the lowest stratum, or any change beyond that of ordinary solidification*.

It seems certain, therefore, that this method of obtaining the heat, by blanketing the surface with strata, is not sufficient.

Neither, as Mallet has observed (p. 303 of the last volume of Silliman's American Journal), can heat be derived from simple pressure or "mechanical compression," as the language of Vose suggests†. But with *movements* in the strata, or progressive plications, such as the metamorphic rocks themselves show they have experienced, then, according to the principle of the transformation of motion into heat, first suggested with reference to metamorphism by Professor Henry Wurtz of New York in 1866‡, and recently applied to volcanoes and demonstrated by Robert Mallet, Esq., the conditions for metamorphism might be complete even with comparatively little help from a rise in the isogeotherms. This result would certainly follow if the heat from motion is great enough, as Mallet appears to show, to produce fusion. Such a cause is capable, as others have urged,

* The arguments here presented are the same that I urged in 1866 (Silliman's American Journal, vol. xlii. p. 252).

† Vose could hardly have intended to say in place of pressure the *motion* produced by pressure; for in one of his paragraphs he attributes the changes distinctly to "the enormous pressure generated in the folding of masses of rock the depth of which is measured by miles;" and this pressure was that of gravitating sediments alone, while the additional heat required came from a rise in the isogeotherms in consequence of surface accumulations. The truth is, that instead of folding generating pressure, the pressure generated the folding; and the movement attending folding was essential to the existence of the heat requisite for metamorphic changes. Thus the views of Vose and Hunt are set aside by Mallet, instead of being, as Professor Hunt says (the last volume of Silliman's Journal, p. 270), "confirmed" by him. In a letter of May 10th to the writer, Mr. Mallet refers to his opposition to Herschel's theory, and adds that he was "rather amused" at finding himself brought forward by Professor Hunt in support of it. Mr. Vose's views are contained in his work on Orographic Geology, published in Boston in 1866 (136 pp. 8vo).

‡ Silliman's Journal, vol. v. p. 385. Professor Wurtz's opinion was first published in a paper on "Gold Genetic Metamorphism," in the 'American Journal of Mining' (New York), Jan. 25, 1868. The paper was read at the Meeting of the American Association at Buffalo in August 1866.

of producing the heat throughout the strata just where it is needed for work. Under it accumulations of strata of like thickness and composition would be differently acted upon according to the three conditions:—(1) the amount of motion, one principal source of heat; (2) the thickness of the series of beds undergoing movement, another source of heat beneath; (3) the amount of moisture present in the beds. Thus widely diverse metamorphic rocks might be made of the same material; and if a region of feebly metamorphic rocks is found to lie side by side with one of thoroughly metamorphic, the strata of the two may have originally been similar and of one and the same geological horizon.

Metamorphism over large areas is thus one of the direct results of the earth's contraction. Solidification is often only a lower stage in the same process; and the reddening of sandstones, as already explained*, is frequently involved with it.

XXIII. *On the Measure of Work in the Theory of Energy.* By

ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

PROFESSOR MAXWELL gives the following definition and measure of work:—"Work is done when resistance is overcome; and the quantity of work done is measured by the product of the resisting force and the distance throughout which that force is overcome" (Theory of Heat, 1871, p. 87).

1. It is to be presumed that when the uniform force F acts throughout the time T in a given direction upon a body which is free to move in that direction, the resistance overcome by the force will be that arising from the *inertia* of the body—in other words, the resistance which the body offers to any change being effected in its state of rest or motion for the time being, and which is always proportional to the force employed in overcoming it. It follows from this, that, under the circumstances referred to, the resistance which is being overcome at each epoch of time, and therefore the work done in equal intervals of time, will be the same throughout the motion. But if the body is at rest to start with, and T is divided into n equal intervals, the work done at the end of the first interval $\frac{T}{n}$, according to the above measure, will be $\frac{1}{2}F^2\frac{T^2}{n^2}$; that done at the end of the second interval will

* Phil. Mag. for July, pp. 49 & 50.

† Communicated by the Author.

be $\frac{1}{2}F^2\left(\frac{2T}{n}\right)^2$; that at the end of the third interval will be $\frac{1}{2}F^2\left(\frac{3T}{n}\right)^2$, &c.: and therefore the work done in the first interval $\frac{T}{n}$ will be $\frac{1}{2}F^2\frac{T^2}{n^2}$; that *during* the second interval will be $\frac{3}{2}F^2\frac{T^2}{n^2}$; that *during* the third interval will be $\frac{5}{2}F^2\frac{T^2}{n^2}$, &c. It has already been pointed out, however, that the foregoing definition of work implies that the amount of work done in equal intervals of time will be the same. It follows, therefore, that the definition and measure of work above propounded contradict each other in the case we have been considering.

2. Suppose a body whose mass is M to be moving in a certain direction with a velocity V_1, and that the force F is applied to the body in the direction of its motion. Professor Maxwell proves that, if during the small time T the body moves through the space s, and has acquired at the end of T the velocity V, we shall have

$$Fs = \tfrac{1}{2}(MV^2 - MV_1^2),$$

an equation, be it remembered, which holds independently of the magnitude of T, provided F be uniform.

If we put $V = V_1 + v$, we shall have

$$\text{work} = Fs = \tfrac{1}{2}M(v^2 + 2vV_1). \quad . \quad . \quad . \quad . \quad (1)$$

Now v is the pure product of the force F acting on the body M during the time T; whence it appears that, adopting the measure of work above proposed, the work done by the force F on the body M in the time T involves the variable quantity V_1, which is entirely independent alike of F, of M, and of T.

3. The right side of the expression (1) will always be positive so long as V_1 and v have the same sign, *i. e.* so long as the directions of the force and the initial velocity conspire. But if the force and initial velocity have opposite directions, and T and V_1 are both finite, the right-hand side of (1) will first be negative; as the motion proceeds it will become zero; and it will finally become and continue positive. It results, therefore, from the above measure of work, that the work done in a finite time by a finite force acting upon a body of finite magnitude which is free to move, may be zero.

4. The proper work of force is to generate or destroy momentum*; and the work done by the force in a given time will

* No doubt force has another effect—that, namely, of causing a body to describe, or of preventing its describing, space: but of these two effects, viz. the description of space and the generation of momentum in any in-

be properly measured by the momentum created or destroyed in that time*.

The measure thus proposed, in fact, differs from the received measure enunciated by Professor Maxwell less than might at first sight be supposed; for when T is very small, (1) becomes

$$\text{work} = Fs = Mv \,.\, V_1.$$

Thus, while I contend that the work done in a short interval of time under the above circumstances is properly measured by the momentum generated during the time, according to the views upon the subject which are generally received it is measured by the product of the momentum generated and the initial velocity V_1—a position the reasonableness of which, I apprehend, it will be found difficult to establish.

5. If, instead of expressing the work done in terms of the force acting and the time during which it acts, we wish to express it in terms of the force and of the space described under its influence, we have only to replace T in the expression FT by its equivalent in terms of the other variables. This, where the body moves from rest, would give us $\sqrt{2MFs}$ as the measure of the work done by the force F on the body M while moving through the space s.

6 New Square, Lincoln's Inn,
July 29, 1873.

XXIV. *Experiments on the Directive Power of large Steel Magnets, of Bars of magnetized Soft Iron, and of Galvanic Coils, in their Action on external small Magnets. By* GEORGE BIDDELL AIRY, *Astronomer Royal, C.B., P.R.S.—With Appendix, containing an Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass. By* JAMES STUART, *Esq., M.A., Fellow of Trinity College, Cambridge*†.

THE only experiments with which I am acquainted tending to throw light upon the distribution of magnetic power in the different parts of a steel magnet are some very imperfect

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definitely small interval of time, the former will be of a lower order of magnitude than the latter; while, of the small space actually described in the interval, all but an indefinitely small portion will have been described under the influence of the velocity from time to time generated during the interval; the residuum immediately due to the direct action of the force, and in no degeee resulting from acquired velocity, being of the *third* order of small quantities at most.

* From this it follows that the work done by the force F acting during the time T on a body which is free to move will be measured by FT.

† From the Philosophical Transactions for 1872, Part II., having been read February 8.

ones by Coulomb in the French Memoirs for 1789 and other years. It appeared to me that it might be desirable to make experiments of a rather more extensive character, and to add some measures of the magnetic effect of galvanic currents, both directly by their immediate action, and indirectly by the amount of magnetic power which they produce inductively in soft iron.

For the measure of permanent magnetism I selected a bar magnet 14 inches long, 1·4 inch broad, 0·35 inch thick; it has not been touched by a magnet for several years, and is likely to be in a state of very permanent magnetism. For the galvanic currents a cylindrical coil was used 13·4 inches long, 1·4 inch in external diameter, and about 0·9 inch in internal diameter; it has, I believe, four layers of wire, each layer having 160 revolutions of the wire. The battery used with it consisted of three cells, with sulphuric acid diluted in the proportion of 1 to 6; the plates were of zinc and graphite, each exposing on each side about 8 square inches; the circuit was always completed about half an hour before the experiments were begun; and a delicate galvanometer was placed in circuit, by which the steadiness of the current was established. A core of iron 0·8 inch in diameter and of the same length as the coil, removable at pleasure, fits well in the inside of the coil; the iron is quite soft, and can with ease be entirely freed from any subpermanent magnetism.

The first step in the experiment was to neutralize terrestrial magnetism within the area of magnetic experiment. For this purpose two powerful 2-feet magnets were placed below the table on which the experiments were made, with their red or north-seeking ends directed to the magnetic north, at a distance (determined by trial) such that the experimental compass was sensibly uninfluenced by terrestrial magnetism. It is possible that some small residual magnetism was perceptible in the comparison with the feeble galvanic action; but none could be certainly discovered in the other experiments.

The compass used for register of the magnetic action is a small and very lively pocket-compass, with needle 1·0 inch long, not loaded with a card. The box of this compass is circular; and when positions had been selected for the centre of the compass (as will be mentioned), a circle somewhat larger than the compass-box was described in pencil with each of those positions for centre; and the compass could then be planted with its centre very accurately placed above the intended point.

The compass-positions were thus prepared:—Upon a sheet of strong paper the plan of the magnet, 14 inches by 1·4 inch, was laid down. On each side were drawn two parallel lines, of

the same length as the magnet, at distances respectively 1·5 inch and 3·0 inches from the near edge of the magnet; these lines were divided each into ten equal parts; and thus in each line eleven points were obtained at intervals of 1·4 inch. From each of the four angles of the magnet as centre, two quadrants were swept:—one with radius 1·5 inch, at whose extremity and bisection points were taken for the compass-centre; and one with radius 3·0 inches, which was twice bisected, and of which the extreme point and the three bisection-points were taken for the compass-centre. These points were used for the magnet both with its edge and with its flat side towards the compass. A similar process was adopted in using the galvanic coil, with this difference only, that the longitudinal separation of the points taken for compass-centre was only 1·34 inch.

A solid piece of wood was provided, in which was cut a concave channel, less than half a cylinder, such that when the galvanic coil, or the large magnet with its flat side towards the compass, was laid in the channel, its axis was sensibly at the same height as the needle of the small compass. With the magnet's edge towards the compass, that condition was sufficiently secured by merely laying its flat side upon the board. The paper with station-points, being laid in proper position upon the board and secured by nails, was cut along the middle of the channel and crosswise at its ends, so that it could be bent down into the channel to permit the magnet or coil to take its proper position. When observations were finished, the paper was detached from the board, and the edges which had been cut were reunited by cementing a piece of paper behind.

The observation (as will be seen) consisted, in every case, of observation of the direction taken by the small needle. And this observation was made solely by the eye. The observer, looking endways of the small needle, made two pencil-dots upon the paper, corresponding to the line of the needle-axis produced as it appeared to his eye. If, from erroneous position of the eye, a parallactic error is produced in the position of the two pencil-dots, this error is detected as soon as the compass has been removed and an attempt has been made to draw a line of direction through the station-point of the compass; and, to correct it, all that is required is, to draw through the station-point a line parallel to the line joining the two dots. The whole of this operation is extremely accurate.

For measuring the intensity of the magnetic force exerted on the compass-needle, I determined, after consideration, to adopt the statical method—that is, to place a constant magnet in a definite position above the compass-needle, with its magnetic

axis transversal to the direction which the compass-needle had taken before the constant magnet was introduced, and to observe the deflection produced. The measure of the force of the large magnet was then the cotangent of the angle of deviation. The observation of the deflected needle by dots &c. was the same as before; but the angle of deviation was never measured by degrees. Instead of that measure, a circle upon semitransparent paper was graduated by cotangents, and thus the measure of the force of the large magnet was read off at once.

The arrangements in this state were confided to Mr. Carpenter, Assistant, of the Royal Observatory, by whom all the subsequent arrangements were planned and all the observations were made. I need not say that they were made with the utmost skill and delicacy.

A small frame was constructed, carrying a floor at a definite position about 1·8 inch above the compass-needle. As it was my object to make the observations at small distances from the great magnet, where its power is great, it was necessary to use a large power in the deflecting-magnet. Mr. Carpenter selected a horse-shoe magnet about 4 inches long, consisting of sixteen plates, each 0·06 inch thick; these were retouched a few days before they were used. From the consistency of the results obtained at the beginning and end of each circuit of the great magnet, I am entitled to conclude that no sensible change took place in the magnetism of the horse-shoe magnet. The magnet was placed in a vertical position, its two poles resting on the raised floor above mentioned. In all cases the deflecting-magnet was used in the two positions to produce deflection right and deflection left.

These arrangements sufficed for observation of the powers of the great magnet in both positions, and also for observation of the galvanic coil carrying the soft-iron core, the intensity of the battery having been in some measure adjusted to make the power of the coil with core not very different from that of the magnet. But when the coil was used without core, the force was so enormously reduced that the arrangement which applied well in the other cases failed totally in this. A small magnet was then used, 1·25 inch long, not very strongly magnetized; its deflecting-power was compared with that of the horse-shoe magnet in the following way:—The small compass being under the influence of the earth's magnetism, the horse-shoe magnet and the small magnet were successively placed on the raised floor above mentioned, then 0·5 inch higher, then 1·0 inch higher; and the cotangents of deflection were compared. Thus the following proportions were obtained:—

Magnets upon the raised floor . . . . . . $\left.\right\}$ $\dfrac{\text{power of small magnet}}{\text{power of horse-shoe magnet}} = \frac{1}{107}$.

Magnets 0·5 inch above the raised floor . . $\left.\right\}$ $\dfrac{\text{power of small magnet}}{\text{power of horse-shoe magnet}} = \frac{1}{136}$.

Magnets 1·0 inch above the raised floor . . $\left.\right\}$ $\dfrac{\text{power of small magnet}}{\text{power of horse-shoe magnet}} = \frac{1}{125}$.

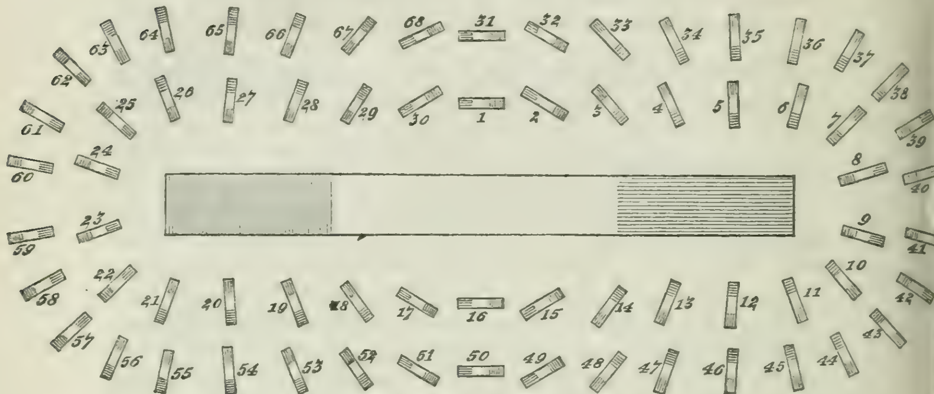
With so great inequality the results are necessarily irregular. I use $\frac{1}{120}$ as the proportion for comparison, without asserting that it is accurate. All results obtained for the coil without core ought therefore to be divided by 120, to make them comparable with the other results.

The results obtained for the direction of magnetical force now consisted of lines drawn upon paper. Upon examining these, some very small irregularities were found, generally of systematic character—partly arising from minute failure in the neutralization of terrestrial magnetism, partly from a difference in the intensity of the poles of the great magnet; these were eliminated by the following graphical process:—The paper was bent upon its longitudinal axis, and exposed to a strong light passing through the two folds of paper; the lines drawn upon both sides of the magnet or coil were visible, and a mean line bisecting the small angle between each pair was easily drawn. Then the paper was unfolded and was bent upon its transversal axis, and a similar operation was performed upon the mean lines mentioned above. Thus for one fourth part of the circumference of the magnet a series of lines was obtained representing the mean of the four parts; these mean lines, repeated for the four divisions of the magnet's or coil's circumference, are alone used in further graphical deductions and in the subjoined figure (p. 226).

The results, however, for magnitude of force were obtained in numbers. The means of these were taken in an analogous order—first taking the sums of those on opposite sides of the magnet or coil, then taking the sums of the last-found sums for opposite ends. The division by 4 was omitted; and thus the numbers in the Table below give the value of 400 × cotangent of deviation. At two stations the proximity of the coil-terminals made it difficult to obtain actual observations; but there was no difficulty in supplying them conjecturally, with great confidence in their accuracy.

The diagram below was drawn carefully to represent the positions taken by the small magnet when the edge of the large magnet is presented to the small magnet. The same diagram will serve, almost without perceptible error, for the case when the flat side of the large magnet is presented to the small

magnet, or for the case of a galvanic coil enclosing an iron core; but it will not apply to the case of a galvanic coil without an iron core; for that case the axis of the small magnet in the positions numbered 35, 5, 12, 46, and all to the right of it



must be directed almost exactly to the centre of the right-hand end of the magnet, and a similar direction must be made at 65, 27, 20, 54 in respect of the left end, with corresponding changes for intermediate stations.

The angles of position were never measured; but they are fully taken into account in the subsequent resolution of forces into longitudinal and transversal components.

The following Table (p. 227) exhibits the total force at each station, as ascertained by the operations described above. It will be remembered that the numbers for the "Galvanic coil with iron core" are not necessarily on the same scale as those for the "Large magnet," and that the numbers for the "Galvanic coil without core" must be divided by 120 to make them comparable with those for the "Galvanic coil with iron core."

Perhaps the following points are worthy of present notice:—

1. Remarking that, in the experiment in which the edge of the large magnet is presented to the small magnet, the distance of the small magnet is in each circuit the same at every station, it will be seen that the greatest directive force is not longitudinal at the end of the magnet, but transversal, at about $\frac{1}{10}$ part (or probably less) of the length from the end of the magnet. There is, however, a diminution and then an increase in proceeding from either of these positions to the other; and the directive force opposite the middle of the magnet's length is less than either of them; so that, in making the entire circuit of the magnet, there are six maxima and six minima.

Total force acting on the small magnet at each station.

No. of Station.				Large magnet presenting its edge.	Large magnet presenting its flat side.	Galvanic coil with iron core.	Galvanic coil without core.
1	16			274	250	310	216
2	15	17	30	326	293	330	240
3	14	18	29	408	363	413	333
4	13	19	28	566	480	515	550
5	12	20	27	678	542	634	1000
6	11	21	26	622	480	585	1470
7	10	22	25	513	454	565	1925
8	9	23	24	600	584	840	2700
31	50			160	160	164	184
32	49	51	68	163	165	180	192
33	48	52	67	191	183	195	225
34	47	53	66	224	200	221	305
35	46	54	65	235	217	227	400
36	45	55	64	211	197	215	427
37	44	56	63	193	180	200	444
38	43	57	62	175	173	195	485
39	42	58	61	181	177	208	583
40	41	59	60	201	193	227	690

2. When the flat side is presented to the small magnets, the same statement holds for the outer circuit, but not for the inner circuit.

3. With increase of distance, the diminution of force is much more rapid at the end than at the side of the large magnet.

4. The law of effect of a soft-iron bar surrounded by a galvanic coil differs, but not greatly, from that of the large magnet presenting its edge. It would seem not improbable that this may depend partly on the effect of the coil which encloses the iron bar; and if so, the law for a soft-iron bar approaches still more to that of a magnet.

5. The exhibition of the effect of the magnetic coil alone is worthy of careful examination. The first thing which will strike the eye is the astounding increase of power produced by the insertion of the soft-iron core. At the sides of the magnet, where the measures of force for the coil alone are 1·5 and 1·8, those for the coil with core enclosed are 164·0 and 310·0; at the ends, where the coil alone gives 5·75 and 22·5, the coil with core included gives 227·0 and 840·0.

6. The law of magnitude of forces for the coil alone differs greatly from that of a steel magnet. In the inner circuit the proportion of the force at the end to force at the middle of length is, for the steel magnet $\frac{600}{274}$, for the coil $\frac{2700}{216}$; in the outer circuit they are $\frac{201}{160}$ and $\frac{690}{184}$.

7. Still more remarkable is the difference in the law of

direction of the forces near the ends. Using the term "pole" to denote that point near the extremity to which the directions of forces rudely converge, the pole of the steel magnet is within the magnet, and distant from the end by about $\frac{1}{12}$ of the magnet's length: but the pole of the galvanic coil is absolutely at its end; indeed some of the experimental directions of force fall a little beyond the end.

It is evident, from the remarks of Nos. 6 and 7, that a magnet cannot in any wise be represented as a system of revolving galvanic currents, with an equal number of circuits at every part of its length.

With the view of presenting the results in the form which may probably be found most advantageous for comparison with the conclusions from any future theory, I have resolved the forces into rectangular directions, parallel and transversal to the axis of each magnet, by the following graphical process. Upon each mean line of direction of force (ascertained as is described above) I have laid down the mean measure of the force (as found above), and upon this measure as hypotenuse I have constructed a right-angled triangle, the lengths of whose sides give the two forces. From the nature of the preceding operations, it is only necessary to form these numbers for one quadrant of each magnet. The results are given in the following Tables.

Large Bar Magnet.

For attracted point.		Edge towards small magnets.		Flat side towards small magnets.	
Longitudinal ordinate.	Transversal ordinate.	Longitudinal force.	Transversal force.	Longitudinal force.	Transversal force.
0·0	2·2	−274	0	−250	0
1·4	2·2	−283	+161	−260	+137
2·8	2·2	−262	+315	−236	+276
4·2	2·2	−198	+530	−182	+444
5·6	2·2	− 56	+678	− 36	+540
7·0	2·2	+216	+585	+158	+451
8·08	1·76	+367	+360	+325	+315
8·5	0·7	+580	+166	+552	+184
0·0	3·7	−160	0	−160	0
1·4	3·7	−147	+ 73	−149	+ 72
2·8	3·7	−127	+142	−123	+136
4·2	3·7	− 88	+205	− 79	+185
5·6	3·7	− 19	+235	− 12	+217
7·0	3·7	+ 49	+205	+ 51	+190
8·13	3·5	+ 95	+167	+ 92	+154
9·12	2·8	+129	+122	+124	+118
9·79	1·84	+159	+ 88	+157	+ 82
10·0	0·7	+199	+ 42	+190	+ 40

Galvanic Coil.

For attracted point.		Coil with iron core.		Coil without core.	
Longitudinal ordinate.	Transversal ordinate.	Longitudinal force.	Transversal force.	Longitudinal force.	Transversal force.
0·0	2·26	−310	0	− 216	0
1·34	2·26	−286	+169	− 240	+ 38
2·68	2·26	−252	+327	− 315	+ 120
4·02	2·26	−193	+477	− 450	+ 325
5·35	2·26	− 42	+632	− 550	+ 848
6·7	2·26	+162	+562	− 80	+1480
7·78	1·70	+380	+420	+1010	+1630
8·2	0·74	+790	+296	+2480	+1170
0·0	3·73	−161	0	− 134	0
1·34	3·73	−162	+ 82	− 189	+ 41
2·68	3·73	−124	+149	− 200	+ 104
4·02	3·73	− 88	+201	− 217	+ 212
5·36	3·73	− 19	+226	− 123	+ 383
6·7	3·73	+ 39	+214	− 57	+ 424
7·83	3·44	+ 94	+176	+ 100	+ 436
8·82	2·8	+134	+149	+ 264	+ 410
9·49	1·82	+186	+ 99	+ 475	+ 338
9·7	0·73	+223	+ 44	+ 668	+ 186

The centre of the large magnet or coil is in every case the origin of coordinates of the external magnetic point on which the action of the large magnet &c. is estimated—the axis of the longitudinal ordinate being the axis of the magnet, and the axis of the transversal ordinate being normal to it. The powers are estimated as those of the red end of the large magnet operating on a small external mass of red magnetism. It will be remembered that, for the galvanic coil without core, all the numbers must be divided by 120.

It does not appear possible to infer from these numbers, by any direct analytical process, the law of distribution of magnetism in the bar. It must be done, I believe, synthetically, by assuming a law, and computing the forces which would result from that law, and then comparing these computed forces with the forces actually observed. The only law which I have tried is the supposition that the intensity of magnetism is proportional to the distance from the centre of the magnet, which includes also the law that there is a gradual increase of red magnetism from one end and a gradual increase of blue magnetism from the other end. Putting l for the half-length of the magnet, a and b for the longitudinal and transversal ordinates of the attracted point, x for the longitudinal ordinate (measured from the centre) of any attracting point, and supposing the magnet to be a line, it is easily seen that the quanti-

ties to be integrated are :—

$$\text{Longitudinal } \frac{-x(x-a)}{\{(x-a)^2+b^2\}^{\frac{3}{2}}}, \quad \text{Transversal } \frac{-bx}{\{(x-a)^2+b^2\}^{\frac{3}{2}}};$$

and the results of integration are :—

$$\text{Longitudinal force} = \frac{l}{\{(l+a)^2+b^2\}^{\frac{1}{2}}} + \frac{l}{\{(l-a)^2+b^2\}^{\frac{1}{2}}}$$

$$+ \text{hyp.log}\,\{\overline{(l+a)^2+b^2}|^{\frac{1}{2}} - l - a\} - \text{hyp.log}\,\{\overline{(l-a)^2+b^2}|^{\frac{1}{2}} + l - a\},$$

$$\text{Transversal force} = \frac{-al-a^2-b^2}{b\{(l+a)^2+b^2\}^{\frac{1}{2}}} + \frac{-al+a^2+b^2}{b\{(l-a)^2+b^2\}^{\frac{1}{2}}}.$$

I have computed these numbers for each of the eighteen stations. For comparison with observation, I have taken the experiments with the flat side towards the small magnets, which represents most nearly the case of a linear large magnet; and, for facility of comparison, I have multiplied the experimental numbers by 6. The following is the comparison :—

Experimental.		Theoretical.	
Longitudinal.	Transversal.	Longitudinal.	Transversal.
−1500	0	−1849	0
−1560	+ 822	−1750	−1089
−1416	+1656	−1441	−2112
−1092	+2664	− 827	−2928
− 216	+3240	+ 155	−3180
+ 948	+2706	+1126	−2283
+1950	+1890	+1589	−1389
+3312	+1104	+2395	− 622
− 960	0	−1029	0
− 894	+ 432	− 971	+ 517
− 738	+ 816	− 776	+ 960
− 474	+1110	− 428	+1267
− 72	+1302	− 2	+1319
+ 306	+1140	+ 335	+1066
+ 552	+ 924	+ 409	+ 801
+ 720	+ 708	+ 668	+ 633
+ 942	+ 492	+ 805	+ 380
+1140	+ 240	+ 984	+ 251

The agreement is not satisfactory; but I am unable to suggest the nature of the change that ought to be made in the assumed law.

I shall add only one remark, of a somewhat practical character. In a paper published originally by Dr. Lamont in Poggendorff's *Annalen*, vol. cxiii. p. 239 &c., and of which a translation, by W. T. Lynn, Esq., of the Royal Observatory, is printed in the Philosophical Magazine, 1861, November,

Dr. Lamont inferred the proportion of the effects of different steel magnets from the proportion of the effects of different soft-iron bars under the influence of induction. The remark No. 4 (above) goes far, I think, to justify this assumption.

Appendix.

Remarking the singularity of the experimental result as to the apparent localization of the attractive pole of a galvanic coil at the very extremity of the coil, I commenced an investigation of the theoretical attraction of a coil, on the laws of galvanic attraction usually received. On my mentioning the subject to my friend James Stuart, Esq., Fellow of Trinity College, Cambridge, he kindly undertook, at my request, to prepare a complete theoretical investigation. I am happy in being permitted by Mr. Stuart to place before the Royal Society his mathematical discussion of the attraction of the coil, which I am confident will be found to be very complete and of great elegance. I append to it a comparison of the numerical results of the theory with the numerical results of experiment; and the agreement will be found to be so great as to justify entire confidence in the assumed law of galvanic action and the mathematical treatment of it, and a high estimate of the accuracy of the experimental observations.

Investigation of the Attraction of a Galvanic Coil on a small Magnetic Mass. By James Stuart, *M.A., Fellow of Trinity College, Cambridge*.*

From investigations given by Ampère, we can deduce an expression for the potential U at an external point Q of a closed circular galvanic current carried by a wire of indefinitely small section. Let a be the radius of the circle, let the distance of Q from C, the centre of the circle, be r, and let the line C Q make an angle θ with the normal to the plane of the circle: then it can be shown that, when r is less than a,

$$U=2\pi k\left\{-1+\frac{r}{a}P_1-\frac{1}{2}\frac{r^3}{a^3}P_3+\frac{1.3}{2.4}\cdot\frac{r^5}{a^5}.P_5-\dots\right\};$$

and when r is greater than a,

$$U=2\pi k\left\{-\frac{1}{2}\frac{a^2}{r^2}P_1+\frac{1.3}{2.4}\cdot\frac{a^4}{r^4}P_3-\frac{1.3.5}{2.4.6}\cdot\frac{a^6}{r^6}P_5+\dots\right\},$$

* Abbreviated from the Appendix originally presented and read with the paper.

where k depends only on the intensity of the current, and where P_1, P_3, P_5 are defined by the equation

$$\frac{1}{\sqrt{1-2x\cos\theta+x^2}}=1+P_1x+P_2x^2+P_3x^3+\ldots.$$

If, therefore, X represents the resolved part, perpendicular to the plane of the circle and towards it, of the force exerted by the current on a unit of magnetism placed at Q, and if Y represents the resolved part of that force parallel to the plane of the circle and directed from its centre outwards, then

$$X=\frac{dU}{r\,.\,d\theta}\sin\theta-\frac{dU}{dr}\cos\theta,$$

$$Y=\frac{dU}{r\,.\,d\theta}\cos\theta+\frac{dU}{dr}\sin\theta.$$

To calculate these quantities, we know that

$$P_1=\cos\theta,$$

$$P_2=\tfrac{5}{2}(\cos^3\theta-\tfrac{3}{5}\cos\theta),$$

$$P_3=\tfrac{63}{8}(\cos^5\theta-\tfrac{10}{9}\cos^3\theta+\tfrac{15}{63}\cos\theta).$$

We shall only consider the case of those points for which r is greater than a. Substituting these values in the expression which in such instances holds for U, we have

$$U=2\pi k\left\{-\frac{1}{2}\cdot\frac{a^2}{r^2}\cos\theta+\frac{15}{16}\cdot\frac{a^4}{r^4}\left(\cos^3\theta-\frac{3}{5}\cos\theta\right)\right.$$
$$-\frac{315}{128}\cdot\frac{a^6}{r^6}\left(\cos^5\theta-\frac{10}{9}\cos^3\theta+\frac{15}{63}\cos\theta\right)$$
$$\left.+\ldots\right\}.$$

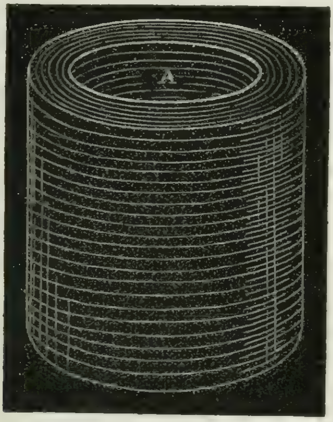
From which, after some reduction, we obtain

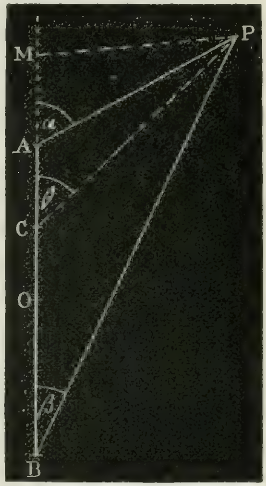
$$\frac{X}{2\pi k}=-\frac{1}{2}(-1+3\cos^2\theta)\frac{a^2}{r^3}+\frac{1}{16}\cdot(9-90\cos^2\theta+105\cos^4\theta)\frac{a^4}{r^5}$$
$$-\frac{1}{128}(-75+1575\cos^2\theta-4725\cos^4\theta+3465\cos^6\theta)\frac{a^6}{r^7}$$
$$+\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1)$$

$$\frac{Y}{2\pi k}=\sin\theta\,.\left\{+\frac{3}{2}\cos\theta\,.\,\frac{a^2}{r^3}-\frac{1}{16}(-27\cos\theta+105\cos^3\theta)\frac{a^4}{r^5}\right.$$
$$+\frac{1}{128}(525\cos\theta-3150\cos^3\theta+3465\cos^5\theta)\frac{a^6}{r^7}$$
$$\left.-\ldots\ldots\right\}\ldots\ldots\ldots\ldots \quad (2)$$

Each of these expressions consists of a series of terms in ascending powers of $\frac{a}{r}$, which will be converging.

We shall now seek to find X and Y for a galvanic current traversing a wire coiled into the form of a hollow cylinder, of which the internal radius is b, the external radius $b+c$, and the length is $2f$. We shall suppose the individual turns of the wire to lie so close as that each may be regarded as an exact circle.





Let A B be the axis of the coil, so that A and B are the centres of its two faces; then $AB = 2f$. Let O be the middle point of A B. Let P be the attracted point, P M its perpendicular distance p from A B. Let $PAM = \alpha$, $PBM = \beta$.

Let C be the centre of any turn of the wire regarded as a circle of radius a, $CP = r$, $PCM = \theta$, $OC = x$; then it is readily seen that for the whole cylindrical bobbin the forces X, Y are given by

$$\frac{X}{\mu} = \int_{-f}^{+f}\int_{b}^{b+c} L dx\, da,$$

$$\frac{Y}{\mu} = \int_{-f}^{+f}\int_{b}^{b+c} M dx\, da,$$

where L and M stand for the expressions on the right-hand side of (1) and (2) respectively, and where μ depends on the strength of the current.

To perform the integrations for the length of the bobbin in

these expressions, we have the formulæ

$$p = r \,.\, \sin\theta,$$

$$\delta x \,.\, \sin\theta = -r \,.\, \delta\theta\,;$$

$$\therefore\ \delta x = \frac{p - \delta\theta}{\sin^2\theta},$$

and

$$r = \frac{p}{\sin\theta}.$$

Making these substitutions for δx and r, the integrals with respect to x become integrals with respect to θ, which can be easily evaluated by a continued application of the method of integration by parts, the limits being from $\theta = \alpha$ to $\theta = \beta$. If we then integrate the result thus obtained with respect to a, from the limit b to the limit $b+c$, we finally obtain

$$\begin{aligned}
\frac{\mathrm{X}}{\mu} = \frac{\overline{b+c}^3 - b^3}{6p^2}&\left\{-(\cos\beta - \cos\alpha) + (\cos^3\beta - \cos^3\alpha)\right\} \\
+ \frac{\overline{b+c}^5 - b^5}{80p^4}&\left\{-\ \ 9(\cos\beta - \cos\alpha) + \ \ 33(\cos^3\beta - \cos^3\alpha)\right. \\
&\left.\quad - \ \ 39(\cos^5\beta - \cos^5\alpha) + \ \ 15(\cos^7\beta - \cos^7\alpha)\right\} \\
+ \frac{\overline{b+c}^7 - b^7}{896p^6}&\left\{-\ \ 75(\cos\beta - \cos\alpha) + 575(\cos^3\beta - \cos^3\alpha)\right. \\
&\quad -1590(\cos^5\beta - \cos^5\alpha) + 2070(\cos^7\beta - \cos^7\alpha) \\
&\left.\quad -1295(\cos^9\beta - \cos^9\alpha) + \ \ 315(\cos^{11}\beta - \cos^{11}\alpha)\right\} \\
+ \ldots\ldots,&
\end{aligned}$$

$$\begin{aligned}
\frac{\mathrm{Y}}{\mu} = \frac{\overline{b+c}^3 - b^3}{6p^2}&\left\{+(\sin^3\beta - \sin^3\alpha)\right\} \\
+ \frac{\overline{b+c}^5 - b^5}{80p^4}&\left\{-\ \ 12(\sin^5\beta - \sin^5\alpha) + \ \ 15(\sin^7\beta - \sin^7\alpha)\right\} \\
+ \frac{\overline{b+c}^7 - b^7}{896p^6}&\left\{+120(\sin^7\beta - \sin^7\alpha) - 420(\sin^9\beta - \sin^9\alpha)\right. \\
&\left.\quad + 315(\sin^{11}\beta - \sin^{11}\alpha)\right\} \\
+ \ldots\ldots&
\end{aligned}$$

These expressions for X and Y will be converging for all points situated at a greater distance than $b+c$ from any point of the axis A B, inasmuch as they are composed by adding together corresponding terms of series which are then all convergent. Among other points, these expressions hold for such as are situated on the axis external to the bobbin, and not nearer A or B than by the distance $(b+c)$. For such points, however, the

expressions become illusory, assuming the form $\frac{0}{0}$; they may, however, be evaluated by the methods for the evaluation of vanishing fractions. Y is clearly zero. X may be more readily obtained directly from the expression for U; from that expression we find that for a single circular current the attraction on such points is

$$X=2\pi k\left\{+\frac{a^2}{r^3}-\frac{3}{2}\frac{a^4}{r^5}+\frac{15}{8}\frac{a^6}{r^7}-\ldots\right\}.$$

Hence, in the case of a bobbin, if x be the distance of the attracted point from O, the middle point of the axis of the bobbin, we have

$$\begin{aligned}\frac{X}{\mu}&=\int_{x+f}^{x-f}\int_b^{b+c}drda\left(+\frac{a^2}{r^3}-\frac{3}{2}\frac{a^4}{r^5}+\frac{15}{8}\frac{a^6}{r^7}-\ldots\right)\\ &=-\frac{\overline{b+c}^3-b^3}{6(x^2-f^2)^2}\left(\overline{x+f}^2-\overline{x-f}^2\right)\\ &\quad+3\frac{\overline{b+c}^5-b^5}{40(x^2-f^2)^4}\left(\overline{x-f}^4-\overline{x-f}^4\right)\\ &\quad-5\frac{\overline{b+c}^7-b^7}{112(x^2-f^2)^6}\left(\overline{x+f}^6-\overline{x-f}^6\right)\\ &\quad+\ldots\ldots,\end{aligned}$$

which gives X for points situated on the axis for which x is not less than $(b+c+f)$.

The expressions for forces which concern us now are those given by the general formulæ for $\frac{X}{\mu}$ and $\frac{Y}{\mu}$. And a moment's glance at these will show that they explain the apparent position of the pole at the very extremity of the coil; for in order to ascertain the values of the forces in a plane at right angles to the axis passing through the extremity of the coil, we must make $\alpha=90^\circ$, $\sin\alpha=1$, $\cos\alpha=0$; and if the other end of the coil be very distant, β may be taken $=0$, $\sin\beta=0$, $\cos\beta=1$. Substituting these values, it will be seen at once that X, the longitudinal force, $=0$, while Y, the transversal force, has a value which indicates a force directed to the extremity of the coil.

In order to make a complete comparison, I have, for all the eighteen stations treated in the former Tables, taken the values of α, β, and p graphically. For b I have adopted 0·45, and for $b+c$ 0·7. These numbers correspond to the internal and external surfaces of the coil; but they appear to me best to represent (though doubtless with some inaccuracy) the quantities used in the theoretical investigation. Then I have (with the kind assistance of Edwin Dunkin, Esq., of the Royal Observatory) made

the complete calculation of the formulæ for every station. As the numbers first obtained were not immediately comparable, I have made them more nearly so by trebling the numbers given by theory and doubling those in the preceding Table. The results are as follows:—

Longitudinal ordinate.	Transversal ordinate, or p.	α.	β.	Result of theoretical calculation.		Theoretical result trebled.		Experimental result doubled.	
				X.	Y.	X.	Y.	X.	Y.
0·0	2·26	161° 10′	18° 40′	− 160	0	− 480	0	− 432	0
1·34	2·26	157 15	15 40	− 168	+ 30	− 504	+ 90	− 480	+ 76
2·68	2·26	151 0	13 25	− 208	+ 82	− 624	+ 246	− 630	+ 240
4·02	2·26	140 20	11 35	− 297	+206	− 891	+ 618	− 900	+ 650
5·36	2·26	121 55	10 15	− 354	+503	−1062	+1509	−1100	+1696
6·7	2·26	91 0	9 20	− 38	+855	− 114	+2565	− 160	+2960
7·78	1·70	58 25	6 10	+ 543	+771	+1629	+2313	+2020	+3260
8·2	0·74	28 0	2 35	+1417	+688	+4251	+2064	+4960	+2340
0·0	3·73	151 0	29 0	− 124	0	− 372	0	− 368	0
1·34	3·73	145 30	24 42	− 128	+ 32	− 384	+ 96	− 378	+ 82
2·68	3·73	137 20	21 30	− 139	+ 77	− 417	+ 231	− 400	+ 208
4·02	3·73	126 10	18 57	− 150	+148	− 450	+ 444	− 434	+ 424
5·36	3·73	110 5	16 58	− 109	+243	− 327	+ 729	− 246	+ 766
6·7	3·73	91 0	15 27	− 20	+295	− 60	+ 885	− 114	+ 848
7·83	3·44	73 0	13 2	+ 80	+308	+ 240	+ 924	+ 200	+ 872
8·82	2·8	54 5	10 0	+ 179	+281	+ 537	+ 843	+ 528	+ 820
9·49	1·82	34 35	6 10	+ 318	+223	+ 954	+ 669	+ 950	+ 676
9·7	0·73	14 25	2 5	+ 456	+120	+1368	+ 360	+1336	+ 372

In spite of some discordances in the large forces (which it was impossible to measure with accuracy), there is enough of agreement to show that confidence may be placed in the method of theoretically computing the attraction of the galvanic coil.

XXV. *On a new Mechanical Theorem relative to Stationary Motions.* *By* R. Clausius*.

IN a memoir published in 1870†, I have, for a material point which moves in a closed path, adduced and demonstrated an equation which stands in close connexion with the proposition of least action and with Hamilton's principle and yet is essentially distinct from them. In the same memoir I then en-

* Translated from a separate impression, communicated by the Author, having been read before the *Niederrheinische Gesellschaft für Natur- und Heilkunde* on June 16, 1873.

† "On the Reduction of the Second Proposition of the Mechanical Theory of Heat to general Mechanical Principles," Phil. Mag. S. 4. vol. xlii. p. 161.

deavoured to apply the equation to the science of heat. The subject, however, appears to me, even from a purely mechanical point of view, to be of so great importance that I have taken the trouble to pursue it further in this direction, and to give the equation as general a form as possible, by which also its application to special cases is of course facilitated and gains in certainty. The result of this investigation I take leave to communicate in the following.

1. It will serve our purpose first to briefly cite the equation in the same form as hitherto in order to connect with it our further considerations.

Given a movable material point of mass m, which, under the influence of a force that has a *force-function* or, according to another nomenclature, *ergal,* moves in a closed path. Let the ergal be denoted by U, the velocity of the point by v, and its period by i. Of the quantities which are variable during the motion the mean value shall be taken, which shall be signified by a horizontal stroke above the symbol representing the variable.

Besides the originally given motion of the point, let us further consider one deviating infinitely little from it. The deviation may be occasioned by the point having begun its motion from another place, or having had at the commencement other components of velocity than with the original motion. Besides, the ergal may have undergone a change. The latter we will imagine expressed by this—that in the function U, in addition to the space-coordinates, one or more quantities c_1, c_2, &c. occur, which are constant during each motion, but may change at the transition from the one motion to the other.

If now, for every quantity that comes into consideration, we regard the difference of the two values which it has in the original and in the deviating motion as the variation of the quantity, and indicate it by δ, and for abbreviation collect the terms which relate to the quantities c_1, c_2, &c. under the sign of summation, the equation in question reads:—

$$\delta\overline{\mathrm{U}} - \Sigma\,\overline{\frac{d\mathrm{U}}{dc}}\,\delta c = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad . \quad . \quad . \quad . \quad (1)$$

2. In order to generalize this equation, it might be assumed that instead of one material point several are given, all moving in closed paths. If all their periods were equal, and changed in the same ratio on the one motion passing into the other, the extension of the equation to such a case would be of itself intelligible at once; but if the periods are different and change in different proportions, special considerations are needed for this extension.

The case is still more general where the points do not describe closed paths, but, though the coordinates of the points change in a periodical manner, the periods have various durations, and the durations may change in different proportions at the transition from the one motion to the other.

This case can be further enlarged thus—that periodical changes are not ascribed to the coordinates themselves, but it is merely assumed that the coordinates can be represented as functions of some quantities which undergo periodical changes.

Finally the treatment can be made still more general, by not directly assuming concerning the quantities by which the coordinates are determined that they accomplish their changes periodically, but fixing a less limiting mathematical condition, which is satisfied by periodical changes, but can also be satisfied without the changes needing to be periodical. This is the method we shall select.

3. Before proceeding to this treatment of our subject, some mechanical considerations may be premised which will facilitate the understanding of it.

Given a system of material points whose masses are m_1, m_2, &c., which move under the influence of forces possessing an ergal. If the positions of the points are determined by the rectangular coordinates x_1, y_1, z_1, x_2, y_2, z_2, &c., the ergal U is a function of these coordinates. The *vis viva* T of the system, if we indicate the differential coefficient of a variable, taken according to the time, by an accent (thus for example putting $\frac{dx_1}{dt}=x_1'$), is expressed as follows:—

$$T=\Sigma\frac{m}{2}(x'^2+y'^2+z'^2). \quad . \quad . \quad . \quad . \quad . \quad (2)$$

As is well known, there is a simple relation between T and U. In order to be able to write this, the sign to be chosen for the ergal U must be fixed more closely. Usually this sign is taken so that the differential of U represents the work done by the forces with an infinitely little displacement of the points, and hence that the proposition of the equivalence of *vis viva* and work is expressed by the equation

$$T=U+\text{constant}.$$

In the form of the proposition, however, which (especially through the beautiful researches of Helmholtz) has more recently come into use, and in which we are accustomed to name it the theorem of the conservation of energy, it is more convenient to introduce the ergal U with the opposite sign, so that the *negative* differential of U represents the work, and hence we can put

$$T+U=\text{constant}.$$

Then T and U are the two quantities which Rankine has named the actual and the potential energy, and whose constant sum is the total energy, or briefly the *energy* of the system. If we denote the latter by E, the preceding equation reads :—

$$T+U=E. \qquad (3)$$

If now, for the determination of the positions of the movable points, instead of the rectangular coordinates any other variables be introduced, which we will denote by $q_1, q_2, \ldots q_n$, of course the ergal U is to be regarded as a function of these variables. As regards the other quantities occurring with the motion, and the equations holding for it, the forms which they assume when those general variables are employed are laid down by Lagrange in his *Mécanique Analytique.*

In order to ascertain what form the expression for the *vis viva* takes, let us put for example, since the rectangular coordinates of the points are to be regarded as functions of those general variables,

$$x=f(q_1, q_2, \ldots q_n).$$

From this follows

$$\frac{dx}{dt}=\frac{df}{dq_1}\frac{dq_1}{dt}+\frac{df}{dq_2}\frac{dq_2}{dt}+\ldots+\frac{df}{dq_n}\frac{dq_n}{dt},$$

or, otherwise written,

$$x'=\frac{df}{dq_1}q'_1+\frac{df}{dq_2}q'_2+\ldots+\frac{df}{dq_n}q'_n. \qquad (4)$$

In like manner can all the velocity-components of the movable points be expressed. As the differential coefficients $\frac{df}{dq_1}, \frac{df}{dq_2}, \ldots \frac{df}{dq_n}$ are functions of the n quantities q, the expressions of the velocity-components contain the n quantities q and the n quantities q', and are, in relation to the latter, homogeneous of the first degree. If we now imagine these expressions put in equation (2), we obtain for the *vis viva* T an expression which also contains the quantities $q_1, q_2, \ldots q_n$ and $q'_1, q'_2, \ldots q'_n$, and in relation to the latter is homogeneous of the second degree.

From the last-mentioned circumstance it follows, further, that we can form the equation

$$2T=\frac{dT}{dq'_1}q'_1+\frac{dT}{dq'_2}q'_2+\ldots+\frac{dT}{dq'_n}q'_n,$$

or, using the sign of summation,

$$2T=\Sigma\frac{dT}{dq'}q'. \qquad (5)$$

As the differential coefficients of T in this equation will frequently recur in what follows, it will be advantageous to introduce for them a simplified symbol. For this we will choose the letter p, and accordingly, understanding by ν any of the integral numbers from 1 to n, put

$$P_\nu = \frac{d\mathrm{T}}{dq'_\nu}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The preceding equation then becomes

$$2\mathrm{T} = \Sigma pq'. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

According to Lagrange, the differential equations of motion take for the general variables q the following form—

$$\frac{d}{dt}\left(\frac{d\mathrm{T}}{dq'_\nu}\right) = \frac{d\mathrm{T}}{dq_\nu} - \frac{d\mathrm{U}}{dq_\nu},$$

or, pursuant to (6),

$$\frac{dp_\nu}{dt} = \frac{d\mathrm{T}}{dq_\nu} - \frac{d\mathrm{U}}{dq_\nu}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

4. As regards the equations given by Hamilton in his memoirs of 1834 and 1835*, they are, if the initial values of the quantities $q_1, q_2, \ldots q_n$ and $p_1, p_2, \ldots p_n$ be denoted by $k_1, k_2, \ldots k_n$ and $h_1, h_2, \ldots h_n$, as follows:—

$$\delta\int_0^t 2\mathrm{T}dt = \Sigma(p\delta q - h\delta k) + t\delta\mathrm{E}; \quad . \quad . \quad . \quad (\mathrm{I.})$$

$$\delta\int_0^t (\mathrm{T}-\mathrm{U})dt = \Sigma(p\delta q - h\delta k) - \mathrm{E}\delta t. \quad . \quad . \quad (\mathrm{I.}a)$$

These two equations are not essentially different the one from the other, because, presupposing the equation $\mathrm{T}+\mathrm{U}=\mathrm{E}$, the one immediately results from the other. Hence they can be designated as one equation in two different forms.

In the first form of the equation the integral

$$\int_0^t 2\mathrm{T}dt$$

is to be regarded as a function of the quantities $q_1, q_2, \ldots q_n$, $k_1, k_2, \ldots k_n$, and E, and the equation can be analyzed into as many different equations as there are independent variations on the right-hand side. As soon as the function which that integral represents is known, we can, from the equations resulting from the analysis, deduce by mere elimination of E all the first and second integrals of the differential equations of motion.

* Philosophical Transactions, 1834 and 1835.

The second form of the equation is still more convenient in the latter respect. In it the integral

$$\int_0^t (\mathrm{T}-\mathrm{U})dt$$

is to be looked upon as a function of the quantities $q_1, q_2, \ldots q_n$, $k_1, k_2, \ldots k_n$, and t; and when this function is known, we obtain through the analysis of the equation alone the first and second integrals of the differential equations of motion.

5. From the preceding it is evident that Hamilton's principle is one of extraordinary importance for mechanics. Nevertheless it is, for two reasons, not suitable for our purpose.

In the first place, great as is its generality in other respects, in one direction it is not sufficiently general. In the equation there are two motions compared which differ infinitely little from each other; and their difference may be reduced to this—that the initial coordinates and velocity-components of the movable points had somewhat different values with the one motion and with the other; but the ergal U is presupposed to be, with both motions, one and the same function of the space-coordinates. The difference, however, between two motions can also be occasioned by the ergal having undergone a change which is independent of the alteration of the coordinates. In the science of heat this case is quite common, because with a body upon which certain external forces act, under the influence of which its molecules perform their motions, these forces may undergo a change which is expressed mathematically by a variation of the ergal, whereby of course a changed molecular motion is necessitated. Transitions of this sort from one motion to another cannot be treated by means of Hamilton's equation.

The second of the reasons above alluded to refers specially to stationary motions. If a stationary motion as such is to be more closely determined, the question is, not to give the positions and velocities of all the individual points for single moments of time, but much rather to fix the general character of the motion independent of time. An equation which is to serve for this purpose may certainly contain variable terms; but their variability must be confined to certain fluctuations of their values, which are repeated in a similar manner, so that the equation has to a later time essentially the same relation as to an earlier one. If, on the contrary, terms occur which are undergoing continually greater variations, so that the equation has not the same relation to a later as it had to a former time, this circumstance makes it unsuitable for our purpose.

We will now consider Hamilton's equation from this point of view. In it occur the variations $\delta q_1, \delta q_2, \ldots \delta q_n$, the significa-

tion of which may be defined thus: δq_ν is the difference between the value which q_ν has with the original motion at a certain instant and the *corresponding* value of q_ν with the deviating motion. But now it is doubtful which of the infinity of values successively taken by q_ν with the deviating motion is to be regarded as the *corresponding* value. Hamilton has certainly said nothing definite on this point; but, by a closer consideration of his developments and equations, one can easily perceive how the variations therein occurring are to be understood. If we commence with the values which the quantities $q_1, q_2, \ldots q_n$ with the original motion have at a certain time t, the corresponding values with the changed motion are those which the quantities have at a time $t+\delta t$—in which the variation δt is not determined, but *the same for all quantities.*

That a common value is indeed to be attributed to the variation δt throughout the system is at once evident from this, that in equation (I.a) δt appears as a quantity valid for the entire system.

Another circumstance which leaves no doubt about this is the following. Hamilton presupposes, in the derivation of his equations, the proposition of the conservation of energy, according to which the sum $T+U$ is constant. But that proposition of course holds only when, in the formation of the quantities T and U, the variables which determine the positions and velocities of the points are brought into the calculation with the values they have at a common time, whether this time be t or $t+\delta t$; but we must not combine values which refer to different times in order to form from them the quantities T and U. Accordingly, with equations thus produced, so long as the opposite is not expressly stated and shown to be admissible, it must be taken as evident that only simultaneously occurring values of all the variables are ever brought into the calculation.

In order to see how variations which correspond to a common time-variation δt behave, we will now select a simple case for consideration. We will, namely, presuppose that with the original motion all the points describe closed paths, and with the deviating motion, again, all the points, starting from infinitely little-changed initial positions, describe infinitely near-lying closed paths, but that the periods of the different points are altered in different proportions.

As the time-variation δt can be taken at pleasure, we will first suppose $\delta t=0$; that is, we will regard as corresponding to one another such values of the variables as belong to one and the same time. If, then, a point has different periods with the two motions, the two positions which belong to one and the same time, reckoned from the commencement of the motion, are so

much further distant from each other as the time is greater. It hence follows that the reciprocally corresponding values of the variables dependent on the positions of the points become continually more different as the time increases; and therefore the variations of these variables do not undergo merely such fluctuations as repeat themselves in a similar manner, but much rather with increasing time ever greater variations of the variables must occur.

If, instead of being supposed $=0$, the time-variation δt be accommodated to the changed period of one of the points, we can thereby cause the variations, at any rate, of those variables which depend only on the position of *that* point to alter only in a periodical manner. For the rest of the variables, however, which depend on the positions of the other points, whose periods have changed in different proportions, there still exists the inconvenience that with time ever greater variations occur, whereby the equation becomes as unsuited to our purpose as before.

6. I now turn to the explanation of the method employed by me for the treatment of stationary motions.

To determine more closely the corresponding values of any quantity Z which varies in the course of the motion, and thereby also to give a more complete definition of the variation δZ, which represents the difference of the corresponding values, we will select a quantity dependent on the time as the *measuring quantity*, and settle that *those values of the variable* Z *which belong to equal values of the measuring quantity shall be regarded as corresponding values.*

If we first take the time itself for the measuring quantity, we obtain the already discussed species of variation, which we will now more closely characterize by putting the measuring quantity t as an index to the δ and consequently writing $\delta_t Z$.

But now, instead of the time t, another quantity ϕ, which changes with the time, may be introduced as the measuring quantity, so that ϕ can be represented as a function of t, or inversely t as a function of ϕ. With the original motion we will first put generally

$$t=f(\phi); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and with the deviating motion, in which the relation between the time and the quantity ϕ can be somewhat different, we will put, the time being for distinction denoted by t^*,

$$t^*=f(\phi)+\epsilon f_1(\phi), \quad . \quad . \quad . \quad . \quad . \quad . \quad (9a)$$

in which f and f_1 represent two yet undetermined functions, and ϵ shall be an infinitesimal constant factor. If now in these two equations the quantity ϕ has the same value, the times t and t^* are to be regarded as corresponding times. If, further, the

above-considered variable has the value Z in the original motion at the time t, and in the deviating motion the value Z^* at the time t^*, Z and Z^* are corresponding values of this quantity, and the difference Z^*-Z is their variation. This kind of variation, in which ϕ is taken for the measuring quantity, shall be denoted by $\delta_\phi Z$. Therefore the difference $t-t^*$, which according to the two preceding equations has the value $\epsilon f_1(\phi)$, has also to be denoted by $\delta_\phi t$.

We have previously represented the time by an undetermined function of ϕ, which on the transition from the one motion to the other undergoes an infinitely little variation. In the nearer determination of this function we can be guided by the nature of the subject of investigation. In the subsequent investigation a very simple form of the function is selected, which is closely connected with the conception of the *phase* introduced in my previous memoir.

To elucidate the notion of the phase, let it be first assumed that the variations which the quantity Z undergoes in the course of the motion proceed periodically, and let the duration of a period be denoted by i. For such a case I have formed the equation

$$t=i\phi, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

and named the quantity thereby defined the *phase* of the variation. With the deviating motion the duration of the periods may be denoted by $i+\delta i$, and then we may put

$$t^*=(i+\delta i)\phi. \quad . \quad . \quad . \quad . \quad . \quad . \quad (10a)$$

If the phase ϕ has one and the same value in both these equations, t and t^* are corresponding times, and we have therefore

$$\delta_\phi t=t^*-t=\phi\delta i. \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

Just so for the quantity Z, those values are corresponding which belong to like phases; and consequently the variation $d_\phi Z$ has a very plain meaning.

Variations of this sort do not with the time take ever greater values, but only change periodically, just as do the quantities themselves of which they are the variations.

[To be continued.]

XXVI. *Analyses of Coal from the Coal-Measures in co. Tyrone, and of a Lignite from Ballintoy, co. Antrim. By* T. CRANSTOUN CHARLES, *M.D. &c., Chemical Assistant, Queen's College, Belfast* †.

THE following results were obtained in the analysis of two specimens of coal from the Coal-measures in co. Tyrone, and of a lignite from Ballintoy, co. Antrim. A series of analyses

† Communicated by the Author.

were made of different portions of these specimens; and as a considerable variability appeared to exist in their composition, so as to show the extremes of this variation, the results of three analyses of three different pieces of each have been given, these having been selected from a number of others closely resembling them.

The analyses were made in the usual way, in a current of oxygen gas, the apparatus being swept at the end of the operation by a current of air. To avoid any loss from the potash-bulbs, the precaution was taken to attach two U tubes, one containing marble and caustic potash, and the other pumice moistened with sulphuric acid. The combustion-tube was more than half filled with copper oxide; and in the remaining part was placed the platinum boat and a long copper boat containing copper oxide, the platinum boat lying between the copper boat and the column of copper oxide. It was found most convenient to use a platinum boat at least 80 millims. long, as the combustion of the coal was thereby greatly facilitated. Not more than ·15 to ·3 grm. was employed for an analysis; and this was taken from a large quantity that had been powdered and well mixed. Before calculating the carbonic anhydride and water from the increase in weight of the potash-bulbs &c., certain corrections were applied. The amounts of these corrections had been previously ascertained by a number of blank experiments, in which known quantities of air and oxygen had been passed through the apparatus, and the weight of carbonic anhydride and water present determined. Though the air and oxygen were first purified by being sent through large U tubes containing caustic potash and sulphuric acid, there was always an increase in weight in the calcium-chloride-tube and the potash-bulbs; it is therefore necessary to apply this correction in accurate experiments.

	(α) Lignite from Ballintoy, co. Antrim.			(β) Coal from 5-foot Main, Drumglass Colliery, co. Tyrone.			(γ) Cannel-coal from Coalisland, co. Tyrone.		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Carbon	47·3	47·8	48·1	73·31	76·5	78·9	73·38	77·58	79·07
Hydrogen ...	6·31	6·25	5·96	7·02	6·42	6·30	6·01	5·53	5·12
Oxygen and nitrogen...	31·49	30·85	30·64	13·77	12·93	10·35	3·44	2·77	2·01
Sulphur	......	......	......	1·6	1·45	1·5	2·3	2·11	1·91
Ash............	14·9	15·1	15·3	4·3	2·7	2·95	14·87	12·01	11·89

(*a*) Wood-like in structure, of a dull brownish-black colour, cuts readily, and does not soil the fingers; particles do not cake when heated; sp. gravity 1·32 to 1·45; contains 17 to 18 per cent. of hygroscopic water. This lignite makes a very good fuel, but is not as rich in carbon as most of the lignites about Lough Neagh, which contain 51 to 58 per cent. (Sir R. Kane). Compared with other lignites its percentage of ash is high; but though it also contains a large quantity of hygroscopic water, yet in this respect it does not contrast so unfavourably with them.

(β) Moderately brittle, of a brilliant black lustre, sp. gravity 1·29 to 1·305, hygroscopic water 7 to 8 per cent., burns rapidly, caking but little, and leaving a light porous coke; a slight admixture of iron pyrites in thin scales; ash light, and of a dull reddish colour. From the large amount of carbon present, and the small amount of ash, this makes a very good burning coal. It is as rich in carbon as many of the English and most of the Scotch coals.

(γ) Hard, compact, difficult to pulverize, of a dull black colour, powder brownish black, burns brightly, tumesces little, cakes but slightly, ash greyish white; sp. gravity 1·275 to 1·37, hygroscopic water 1·5 to 1·9 per cent., coke about 50 per cent. There is a considerable admixture of iron pyrites, which occurs in thin layers. This coal yields a large quantity of gas of high illuminating quality; and were it not for the large percentage of ash it contains, it would be equal to any of the English Cannel-coals.

XXVII. *On the Nodal Lines of a Square Plate.* By Lord Rayleigh, *F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I REGRET to find that the investigation of one of the nodal lines of a square plate, published in your August Number, is to a great extent vitiated by the erroneous assumption that it is possible for a square plate to vibrate after the manner of a simple bar. The function representing such a motion satisfies the general differential equation, as well as the two necessary boundary conditions along the pair of edges for which the motion is constant; but on the other pair of edges one of the conditions is in general violated. This corresponds to the fact that if a plate is bent by couples acting along one pair of opposite edges, it will in general take a contrary curvature in the perpendicular direction.

Several too confident assertions in the paper referred to must now be retracted; and the deviation of Dr. Strehlke's measurements from my calculations are no greater than are fairly attributable to the imperfections of the latter. It appears that the

form of the nodal lines is not (as I had supposed) entirely independent of the relation between the two elastic constants.

I may, however, be permitted to remark that my calculation, though not strictly applicable to a glass or metal plate, belongs to an extreme case of the true solution; for it would be correct if the nature of the material composing the plate were such that the extension produced by a longitudinal force acting along a bar of it were unaccompanied by lateral contraction. This condition of things, though probably not realized in nature, is approximated to in the case of substances such as cork (Thomson and Tait's 'Natural Philosophy,' § 685).

I remain, Gentlemen, your obedient Servant,

RAYLEIGH.

XXVIII. *Reply to some Remarks by* Professor Challis*, "*On Objections recently made to the received principles of Hydrodynamics.*" *By* ROBERT MOON, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

I AM glad to find that my argument in proof of the equation $p=$ funct. (ρ, v), applicable to fluid motion in one direction, has received the sanction of Professor Challis. When personal friends have concurred with opponents in regarding such a relation between the pressure, velocity, and density as a wild suggestion, unworthy of a moment's consideration, it will be readily understood that I am not insensible to the value of support.

Professor Challis labours under a misapprehension, however, when he asserts that the "general theorem includes the more particular case in which the pressure is a function of the density only," and that, therefore, by my "own showing, it is legitimate to make the hypothesis that the pressure is always in exact proportion to the density."

For motion in one direction it is axiomatic that we must have in *every* fluid, in *every* case of motion,

$$p=f_1(xt), \qquad \rho=f_2(xt), \qquad v=f_3(xt);$$

whence it follows that, for every value of x and t for which each of the foregoing equations represents a substantive relation between the variables, we shall have

$$p= \text{funct.}\ (\rho, v).$$

It is not axiomatic, however, though it happens to be true, that in *every* fluid for *particular cases* of motion we may assume

$$p=f(xt), \qquad \rho=a^2f(xt);$$

where a^2 is a constant whose value or values depend on the nature of the particular fluid dealt with.

And it is neither axiomatic nor true, that in *any particular*

* See Phil. Mag. for August. † Communicated by the Author.

fluid whatsoever we shall have *always, or generally,*

$$p=f(xt), \qquad \rho=a^2 f(xt).$$

For proof of these latter propositions I must refer to the integral (elsewhere derived*) of the equation of motion,

$$0=\frac{d^2y}{dt^2}+\frac{1}{\mathrm{D}}\frac{dp}{dx}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

viz. the trio of equations,

$$\left.\begin{aligned} p&=-\frac{\alpha^2}{\rho}+\phi\left(v+\frac{\alpha}{\rho}\right), \\ v+\frac{\alpha}{\rho}&=\psi_1\left\{x-\frac{\phi'(u)-\alpha}{\mathrm{D}}t\right\}, \\ \frac{1}{\rho}+\int\frac{du}{\phi'(u)-2\alpha}&=\psi_2\left\{x-\frac{\alpha}{\mathrm{D}}t\right\}; \end{aligned}\right\} \quad . \quad . \quad (2)$$

where $u=v+\frac{\alpha}{\rho}$, where α is an arbitrary constant, and ϕ, ψ_1, ψ_2 are arbitrary functions†. Suppose that when $t=0$, ρ and v are represented by the equations $\rho=f_2(x)$, $v=f_3(x)$; then we shall have, when $t=0$, from the first of (2),

$$p=-\frac{\alpha^2}{f_2(x)}+\phi\left\{f_3(x)+\frac{\alpha}{f_2(x)}\right\}. \quad . \quad . \quad . \quad (3)$$

Hence, although $p=a^2f_2(x)$ is an admissible assumption here, since by means of ϕ we can satisfy the equation

$$a^2f_2(x)=-\frac{\alpha^2}{f_2(x)}+\phi\left\{f_3(x)+\frac{\alpha}{f_2(x)}\right\},$$

yet the number of different expressions for p which are equally admissible is simply infinite—since, if in (3) we put $f_1(x)$ for p, where f_1 denotes any function whatever, the equation so resulting may equally be satisfied by properly assuming ϕ. The above establishes that there is *no* fluid for which always, or generally, $p=a^2\rho$. But, dismissing for the present this argument, if in general we have $p=$ funct. (ρ, v), I would ask upon what philosophical principle can we be called upon to accept, or even be invited to collect evidence in favour of, the law $p=a^2\rho$, in support of which not the vestige of an *à priori* argument can be adduced, to the entire exclusion of a countless number of other laws which *à priori* have precisely the same claim upon our attention?

* See Phil. Mag. vol. xxxvi. p. 27. I regret that, in two subsequent papers in the Philosophical Magazine, ρ is erroneously written for D in the denominator of the fraction which occurs on the right-hand side of the second equation of the solution.

† That equations (2) satisfy (1) may be verified by differentiating with respect to both x and t either of the last two of equations (2); then eliminating ψ'_1 or ψ'_2, as the case may be, from the equations so arising, and, finally, by eliminating ϕ' from the last result by means of the derivative with respect to x of the first of equations (2).

With respect to the case I have proposed regarding the piston and the weight, of which Professor Challis has treated, he appears to have failed in catching the point of my objection. Admitting for the moment that the piston would begin to descend with the accelerating force $\frac{mg}{M+m}$, with what accelerating force will the stratum of air begin to descend according to the ordinary theory?

At the beginning of the motion the pressure on the upper side of the stratum would be Mg, and that on the under side would equally be Mg. Hence, while at the beginning of the motion the piston, according to the received theory, would have a tendency to move represented by the accelerating force $\frac{mg}{M+m}$, the stratum in immediate contact with the piston at the same epoch would have no tendency to move whatever, a state of circumstances which I maintain to involve a contradiction. If the introduction of the second weight does not produce in the stratum under consideration a tendency to move, what other circumstance can occur which would produce in the stratum such a tendency? If it be answered that this will happen when the piston has descended through an indefinitely small space, I reply that the piston cannot descend through an indefinitely small space without the underlying stratum of air descending through a corresponding space, and that it is impossible that a stratum which has no tendency to move can descend through even an indefinitely small space.

I am afraid that I cannot agree with Professor Challis as to the bearing upon Boyle's law in the case of motion of the cases I have suggested where the density varies *per saltum*. If on one side of a given plane we have a density 2D, and on the opposite side a density D, then without waiting or attempting to decide what the motion would be under such circumstances, it appears to me that I cannot err in saying that, according to the received theory, the pressure of the first portion of the fluid on the second is double that of the second on the first, and that therefore to assert that Boyle's law and the law of the equality of action and reaction hold in this case is simply to maintain a contradiction in terms.

If the views I have endeavoured to unfold are correct, it will follow that the true function of experiment in determining the motion we have been considering is to determine the initial values of the pressure, density, and velocity. These being known, the values of the arbitrary functions ϕ, ψ_1, ψ_2 can be ascertained, and the motion will be completely represented by the equations (2).

P.S.—It may be remarked that, if it be intended that we shall

have $p=a^2\rho$ *throughout* the motion, it will not be enough that we have $p=a^2f_2(x)$ when $t=0$. We must have

$$a^2\rho=-\frac{\alpha^2}{\rho}+\phi\left(v+\frac{\alpha}{\rho}\right).$$

Eliminating $v+\frac{\alpha}{\rho}$ between this equation and the last two of (2) we shall obtain *two* equations between ρ, x, and t only, which must needs be identical; whence it can easily be shown that we must have

$$\frac{\phi'(u)-\alpha}{D}=\frac{\alpha}{D'},$$

and therefore

$$\phi(u)=2\alpha u+C.$$

6 New Square, Lincoln's Inn, August 7, 1873.

XXIX. *Notices respecting New Books.*

The Mare Serenitatis: its Craterology and principal features. By W. R. Birt, *F.R.A.S.* London: Taylor and Francis.

IN the year 1867 Mr. Glaisher, in his report of the Lunar Committee of the British Association for the Advancement of Science, has this remark:—"Had the *Mare Serenitatis* undergone the close scrutiny in the course of this work which the *Mare Crisium* has in compiling the table in the report presented at Birmingham, this Committee could have pronounced decisively as to the state of *Linné* a little anterior to the announcement of change by Schmidt." If the present unsettled state of the question of change, especially as regards *Linné*, be a reproach to Selenography, as it evidently must be when we find a popular writer announcing that an undoubted change in this remarkable crater had really taken place, and then some few years afterwards acknowledging that when he wrote in 1867 he "had not an adequate conception of the difficulties of the subject," it becomes exceedingly important that difficulties which tend to retard the progress of our knowledge of the moon's surface should be met in such a way as to reduce controversy to its smallest extent. This Mr. Birt, in the monograph before us, has endeavoured to do by specifying no less than 333 distinct objects on or near the *Mare Serenitatis*, an extensive lunar plain of more than 400 miles diameter. The map, which has been thoroughly revised, contains every object which has been recorded since Hevel's and Riccioli's epoch to the present; and students who really wish to contribute to the settlement of the interesting question of change the monograph cannot fail to assist materially. The authority by whom any given object was first recorded is inserted in the table accompanying the map; and to each object of special interest illustrative notes are appended. By the aid of the monograph in connexion with existing records, the history of any single object can be readily traced, and its existence or non-existence, with any changes it may have undergone, ascertained.

XXX. *Intelligence and Miscellaneous Articles.*

ON THE PASSAGE OF GASES THROUGH COLLOID MEMBRANES OF VEGETABLE ORIGIN. BY A. BARTHÉLEMY.

THE purpose of these experiments was to prove that those of Graham on the dialysis of gases through caoutchouc could be verified on natural vegetable colloid films, and principally on the cuticular surfaces of the leaf, and thus to justify the important part which I make the cuticle play in the absorption of carbonic acid by plants.

Every body knows the Begoniaceæ with leaves spotted with white, which are cultivated in greenhouses, and the white spots of which are, as I have convinced myself, only elevations of the epiderm upon a layer of nitrogen. The leaves of certain varieties, very thin on the living plant, are reduced, on fading during the winter in darkness, to the condition of a pellicle endued with elasticity and representing almost nothing but the cuticular layers. It was these colloid membranes which served me in repeating Graham's experiment. I followed strictly the course of the illustrious physicist, save a few modifications of detail.

One commences by ascertaining that the membrane is intact, presenting no rents, by the dialysis of air alone. Three experiments, made on the 16th, 17th, and 18th of March, gave the following results at the end of six hours:—

	March 16.	March 17.	March 18.
Volume of gas collected	5·2 cub. cent.	5·5 cub. cent.	7·0 cub. cent.
Volume of oxygen absorbed by pyrogallate of potass..........	1·9 ,,	2·3 ,,	2·2 ,,
Proportion of oxygen ..	36 per cent.	41 per cent.	31 per cent.

Although the proportions of oxygen present a rather wide deviation, due to the difficulty of repeating the experiments under the same conditions of external pressure, temperature, and, especially, of hygrometric condition, it may be concluded that oxygen passes more rapidly than nitrogen, and that air thus dialyzed contains on the average 36 per cent. of oxygen. This number is a little below that found by Graham with caoutchouc.

Having made this verification and obtained this important result, I proceeded to the comparison of the velocities of the three gases which most interest us. For this purpose, having set up a current of carbonic acid above the membrane, I noted the point to which the mercury had descended at the expiration of an hour; then, passing nitrogen or oxygen, I noted the time taken by the mercury to descend to the same level.

In four experiments, with different membranes, I obtained the following results:—

	1st exp.	2nd exp.	3rd exp.	4th exp.
Carbonic acid	1 hour	1 hour	1 hour	1 hour
Nitrogen	15 ,,	$13^h\ 40^m$	$15^h\ 30^m$	14 ,,
Oxygen	6 ,,	$6^h\ 20^m$	7^h	$5^h\ 40^m$

These experiments, made under conditions of pressure, temperature, and hygrometric condition which cannot have been identical, are nevertheless sufficiently concordant with those of Graham, and permit me to conclude that the natural colloid surfaces of vegetables have for carbonic acid an *admissive power* which is from 13 to 15 times as great as that which corresponds to nitrogen, and six or seven times as great as that which refers to oxygen.

A few days afterwards I operated with perfectly dry carbonic acid, and only found, as the velocity relative to nitrogen, numbers varying between 9 and 11. It appears, therefore, that carbonic anhydride passes less quickly than hydrated carbonic acid.

On replacing the vegetable membrane by caoutchouc, I obtained a similar result. The difference produced by dried oxygen and nitrogen is less pronounced.

I will remark, in conclusion, that these experiments prove the dialysis of carbonic acid through the cuticle of leaves, just as much as the experiments of Dutrochet on membranes and aqueous solutions prove endosmose by cellules, or the experiments on absorption made by M. Dehérain with porous vessels, to which the Academy accorded one of its highest rewards. In a word, *cuticular* respiration appears to me sufficiently proved by the presence of this membrane on all the organs, by the analogies of constitution, physical and chemical, with caoutchouc, by Graham's experiments and the measurements of the passage of gases through colloid membranes, and, lastly, by the experiments of M. Boussingault, who attributes to the upper surface of leaves, destitute of stomata, a greater decomposing faculty than that of the lower surface riddled with these minute apertures.—*Comptes Rendus de l'Académie des Sciences*, Aug. 11, 1873.

ON THE REFLECTION OF LIGHT INVESTIGATED BY M. POTIER. BY G. QUINCKE.

The *Comptes Rendus* of the Paris Academy, Sept. 9th and 16th, 1872 (vol. lxxv. pp. 617 & 674), contain two communications from M. Potier on the reflection and refraction of light at the boundary between two media.

He has, with the sodium-flame, observed Newton's coloured rings in a thin plate of glass the hinder surface of which was at the same time bounded by air or sulphide of carbon, or in thin lamellæ between a glass lens and a metallic mirror. He moreover investigated the interference of two pencils of rays, one of which has undergone a metallic, and the other ordinary or total reflection at the base of a glass prism, in its interior. In some experiments the metallic mirror was replaced by a plane glass mirror beneath the surface of the lens, or by a liquid at the outer face of the prism.

M. Potier is of opinion that the æther in the two media was divided by a transition-layer, and that the process of reflection or refraction may be conceived to take place within the transition-layer of two transparent media, in a plane parallel to their common boundary. This plane, he thinks, which formed the optical boundary of the two media for light polarized parallel to the plane

of incidence, was situated at different distances from the common boundary when, one of the two media being left constant, the other was altered in such wise that the optical thickness of a thin lamella varied with the nature of the substances adjacent to it. With glass and air or sulphide of carbon, the difference between these distances was found by experiment to be $=\frac{1}{20}$ wave-length for light polarized parallel to the plane of incidence. The phase of the reflected light undergoes different variations with different incidence-angles, being O with grazing incidence and having a maximum value with normal incidence. The change of phase varies with the nature of the thin lamella between the lens and the plane mirror. The ray reflected from metal is retarded when the reflection is normal, the retardation amounting to $\frac{1}{9}$ of a wave-length for silver and air, to $\frac{1}{6}$ for silver and an essential oil. This retardation has a sensible influence when the thickness of a thin layer of metal upon a glass plate is determined from the different diameters of the Newton's rings formed beneath a lens when laid upon a plate of metal and when laid upon a plate of glass.

"Moreover, with a retardation of the reflected ray, there was a corresponding change of phase in the transmitted ray; and in passing through a transparent lamella of silver, the phase must have been accelerated $\frac{1}{10}$ of a wave-length. This must be taken account of in the determination of the velocity of light in the interior of metals; and this circumstance accounts for it if certain experimenters have found the refraction-index of metals too small or negative. The phenomena in metals were more complicated, as there did not exist here, as in transparent substances, a plane, parallel to the boundary, in which the incident and the reflected ray, both polarized parallel to the plane of incidence, coincide, but only a plane in which the two had a definite difference of path. The situation of this plane varies with the nature of the substance over the metal, since the extinguishing-force of the metal, and, in a less degree, the nature of the substance above the metal, varied the constants of the elliptic polarization produced by metallic reflection—that is, the principal angle of incidence and the principal azimuth."

As, to my knowledge, no one except me * has investigated the refraction-indices of transparent layers of metal, I must refer to myself the expression "certain experimenters," and remark that to the above statements, save so far as they contain what was already known, I cannot assent.

My optical experimental investigations published in the *Annalen* contained, besides those methods which are described in the above-mentioned communications, some others, which have concordantly conducted to this result—"that, in order to be in accord with the known facts, we have to make the assumption that reflection and refraction take place in a transition-layer the thickness of which can be measured by experiment" †.

* Pogg. *Ann.* vol. cxx. p. 599: 1863.
† Ibid. vol. cxli. p. 398: 1871.

In like manner the law given by our author, that reflection takes place in a plane parallel to the boundary surface of transparent substances, is not new. Assuming the change of phase on the passage through the boundary surface of transparent substances to be insensible, as all investigations hitherto appear to prove, that law was established more than twenty years since, by Stokes *, with the aid of the principle of reversion, without any special assumption of a dynamic theory of light under the presupposition that the effective forces depend merely on the situation of the particles in motion.

In accordance with this law is the fact which I have established by experiment, "that with the same reflecting boundary surface of two transparent substances, for corresponding angles of incidence (which are mutually related as angles of incidence and refraction), whether the reflection take place in the one or the other medium, the observed values of the difference of phase of the two ray-components polarized parallel and perpendicular to the plane of incidence are equal and contrary, and the amplitudes of the two components are in the same ratio.

"If the reflection is positive in the one medium, it is negative in the other, and *vice versâ*" †.

The distinction, originated by Jamin ‡, of transparent substances into those with positive and those with negative reflection (that is, with which the component polarized parallel to the angle of incidence is accelerated or retarded in comparison with the perpendicularly polarized component), cannot any longer be maintained, although it has recently been so in German manuals of physics.

That the phase of the reflected light in the case of grazing incidence undergoes no change cannot be decided by the method made use of by our author. By interference of the direct and in the most decided manner grazingly reflected rays, I have proved § that the change of phase is not, as M. Potier states, $=0$, but corresponds to half a wave-length, both for the light polarized parallel and that polarized perpendicular to the plane of incidence.

For other incidence-angles than 90° the difference of the alteration of phase in two differently reflected rays, *e. g.* from glass and metal, can be only relatively determined; and I have determined by experiment, both with the method made use of by our author and with some others, this difference for reflection in different substances, and have discussed in detail how far they deviate from the variety of change of phase required by the theory ‖. Abstraction made of the uncertainty (there, p. 226, fully discussed) of the magnitude of half a wave-length, the observations generally, but not always, give a maximum of difference of phase of two dissimilarly reflected rays when $J=0$. The ray reflected from a metal was, relatively to that reflected from glass, accelerated when the

* Camb. and Dubl. Math. Journal, vol. iv. p. 1: 1849.

† Pogg. *Ann.* vol. cxxviii. p. 369: 1866.

‡ *Ann. de Chim.* vol. xxix. p. 303: 1850.

§ Pogg. *Ann.* vol. cxli. p. 223: 1871.

‖ Pogg. *Ann.* vol. cxli. pp. 196–232, 384–388: 1871.

reflection was in air. There also the results of the earlier investigations by Airy* and Glan† of Newton's rings between lenses and plane mirrors of various materials are treated of, as well as the influence exerted upon experiments of this sort by condensed vapour or gas films on the reflecting surfaces. I would seek in such impurities the reason of the difference between my measurements and those of other observers.

Before the appearance of the communications in question, I had pointed out‡ "that the amplitude and phase of light in passing through thin layers of metal are changed simultaneously, the change depending on the thickness of the metal," and also "that the change of phase in refraction must vary also with the position of the plane of polarization." At the same time, I showed how, independently of the sources of error of the formerly usual methods, a thin lamella of metal displaces interference-streaks as if the refraction-index in the interior of the metal were <1§. On the same occasion it was explained how far it is justifiable to assume an acceleration of the phase when light passes through thin, transparent layers of metal, which M. Potier seeks to establish on theoretical grounds.

When a lens is pressed upon a glass plate partially covered with a thin layer of metal, Newton's colour-rings upon the metal appear displaced towards those upon the glass plate. The thickness of the metal cannot be calculated from this displacement, unless, as is correctly remarked in the communication mentioned, the changes of phase with reflection at the surfaces of the metal and the glass, or the difference between the two, be known.

Hence I have never made use of this method for determining the thickness, but have pressed the lens upon the uncovered part of the glass plate until the lower surface of the lens touched the margin of the sharply bordered layer of metal. Contact was shown by the distortion of the colour-rings on the glass. The colour which was exhibited by the layer of air of the same thickness as the metal *on the glass plate* gave then the thickness of the latter‖. It is at once seen that in this determination the knowledge of the change of phase with normal reflection is not requisite; and therefore ignorance of it could not occasion any error in the rest of the conclusions deduced by me.

Finally, I have also shown how, in reflection from the same metal, the principal angle of incidence and principal azimuth, as well as the qualities generally of the reflected light, depend on the nature of the transparent medium in which the reflection takes place¶, and to what depth light of different colour and plane of polarization penetrates the metal; so that, in relation to this also, M. Potier's remarks cannot be said to be new.—Poggendorff's *Annalen*, vol. cxlviii. pp. 311–316.

* Cambr. Trans. vol. iv.; Pogg. *Ann.* vol. xxvi. p. 123: 1832.
† *Absolute Phasenänderungen durch Reflexion*: Berlin, 1870.
‡ Pogg. *Ann.* vol. cxli. p. 191: 1871.
§ Ibid. p. 186: 1871.
‖ Ibid. vol. cxxix. p. 178: 1866.
¶ Ibid. vol. cxxviii. p. 547 *sq.*, cxxix. pp. 182*en*217: 1866.

EXPLOSIONS PRODUCED BY HIGH TONES.

A great part of the known explosive substances contain more or less nitrogen. The simplest of these, and at the same time one of the least stable, is the combination of iodine with nitrogen. Iodide of nitrogen is very easily prepared by wetting finely powdered iodine with liquid ammonia. It is then filtered; and the filter, while still moist, is taken out of the funnel and cut up into small pieces, which are dried separately. Although this substance while moist is quite harmless, yet as soon as it is dry it detonates with great violence on the slightest friction. But what is most remarkable is, this violent decomposition can be called forth by certain high tones.

MM. Champion and Pellet have made the following interesting experiments upon it:—Two glass tubes of 15 millims. diameter, and 2·4 metres total length, are combined by means of a strip of paper; and upon each end is put a little piece of paper which contains 0·03 of a gramme of iodide of nitrogen. When one of these is caused to detonate, the other detonates likewise.

The explosion of the second paper, however, is not effected by pressure of the air: this can be shown by introducing a small pendulum into the tube; it is no more disturbed by the explosion than when the tube is blown strongly into with the mouth. If we fasten such papers to the strings of a double bass, or a violoncello, or a violin, we can demonstrate that the deep tones exert no action, while the high tones induce a detonation. The very high tones obtained by twitching the strings on the tail side of the bridge give the same result.

The results were similar in experiments with Chinese tomtoms: the deep-toned instruments effected no detonation; the high-toned ones constantly did so.

Two parabolic concave mirrors of 0·5 of a metre diameter were set up at 2·5 metres distance one from the other. A small quantity of iodide of nitrogen was brought into the focus of one of them, and another small quantity was placed in the centre between the two mirrors. Some nitroglycerine was exploded in the focus of second miror: the iodide of nitrogen in the focus of the first mirror detonated; that in the centre remained intact. Although other explosive substances placed in the focus of the second mirror produced the same effect, yet this was not, as might perhaps be thought, a consequence of heat, because 0·03 gramme of nitroglycerine, which does not develop more heat than 0·9 gramme of gunpowder, produced an explosion similar to that produced by from 8 to 10 grammes of powder.

The mirror was then coated with lampblack, when the explosion of 10 grammes of powder did not induce any detonation of the iodide of nitrogen, while 0·03 gramme of nitroglycerine was still sufficient to do so.—*Chronique de l'Industrie*, No. 52, Jan. 29, 1873.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

OCTOBER 1873.

XXXI. *On a Relation between Heat and Static Electricity.* *By* Frederick Guthrie*.

THE experiments to be described concern a remarkable relation between heat and static electricity, and may be briefly described as exhibiting (1) the difference in power which one and the same body at different temperatures possesses of discharging electricity of one kind, and (2) the difference in power which one and the same body at the same temperature has of discharging the two kinds of electricity.

A great many of my experiments involved the use of white- or red-hot metal balls. Those I used were the ordinary cast-iron balls about two inches in diameter, provided with eyes. An insulating handle was made by fastening a stout copper wire hook by means of thinner wire to the end of an ebonite pen-holder about six inches long. When no insulation was required an ordinary iron hook was used. The iron balls were heated in a clear coke fire. Each experiment was made never less than twelve, and mostly from twenty to thirty times, on different days. The two gold-leaf electroscopes which I used kept their charge without sensible loss for from five to ten minutes.

§ 1. *Experiment*.—A white-hot iron ball being hooked out of the fire by the insulating hook and brought into contact with the + conductor of an electrical machine, and then brought near to or touching the top of an electroscope, produces no divergence of

* Communicated by the Author. (An abstract of this investigation appears in the 'Proceedings of the Royal Society,' February 13, 1873: vol. xxi. p. 168.)

the leaves. It is in fact impossible to use a white-hot iron ball as a "proof plane" for + electricity.

§ 2. *Experiment.*—A white-hot iron ball being hooked out of the fire, as in § 1, by the insulating hook, is made to touch the − conductor of the machine; it is then found to be incapable of influencing the electroscope by induction or by contact. The white-hot ball cannot serve as a proof plane for − electricity.

§ 3. *Experiment.*—On the top of the electroscope is placed a wooden block covered with tinfoil. Above this are a few sheets of metal plate. Upon these is a tripod stand inverted, to which a metal chain is fastened. The other end of the chain can be connected with the + or − conductor of an electric machine. A white-hot iron ball is placed on the tripod, and the chain is fastened to the + conductor. As long as the machine is worked the leaves diverge; but on ceasing, they immediately drop together.

§ 4. *Experiment.*—The arrangement being precisely the same as in § 3, the chain is fastened to the − conductor of the machine. As before, the leaves only remain divergent while the machine is being worked.

§ 5. *Experiment.*—The chain in § 3 being removed, the white-hot ball is placed upon the tripod. It is found impossible to make the leaves diverge by a charge from a proof plane of 24-inch surface which has been charged by either the + or − conductor and then brought into contact with either the white-hot ball or any other part of the electroscope's system.

§ 6. *Experiment.*—A white-hot iron ball on a metal hook (so as to be in earth-connexion) is held above the top of an electroscope charged with + electricity. When at the distance of one or two inches, the leaves rapidly and completely collapse, and do not recover their divergence on the removal of the ball.

§ 7. *Experiment.*—The electroscope is charged with − electricity, the conditions being otherwise as in § 6. When the ball is at a distance of three or four inches the electroscope is rapidly, completely, and permanently discharged.

§ 8. *Experiment.*—The electroscope being charged with + electricity, the white-hot iron ball is hooked out of the fire with the insulating hook and brought above the electroscope. Perfect and permanent discharge takes place when the interval between the two is about one inch.

§ 9. *Experiment.*—The electroscope is charged with − electricity, the experiment being otherwise as in § 8. The electroscope loses its charge completely and permanently when the interval is two or three inches—that is, sensibly greater than with + electricity.

§ 10. *Experiment.*—An iron wire with a little cotton-wool fastened to one end is dipped into benzol, ether, alcohol, or bi-

sulphide of carbon; the liquid is ignited, and the flame is held above the electroscope charged with + electricity. At a distance of three or four inches immediate perfect and permanent discharge takes place.

§ 11. *Experiment.*—The same flames are found to discharge the − electricity of an electroscope at about the same distance.

§ 12. It will be noted that these flames included white luminous smoky (benzol), white luminous (ether), feebly luminous (alcohol and bisulphide of carbon). They are all water-forming but the last. There is little, if any, difference noticeable in their power of discharge. A paper spill or wooden splinter answers almost equally well.

§ 13. *Experiment.*—The wire holding the cotton-wool saturated with the liquids of § 10 is fastened to an ebonite penholder, so that the flames are not in electric contact with the earth. The power of discharging both + and − electricity of an electroscope appears to be sensibly the same as when there is earth-connexion, the discharge being quick, complete, and permanent at a distance of three or four inches.

§ 14. The success of the above experiments with white-hot balls depends upon the balls being of a bright white heat; the hotter the better. Complete success can be obtained in an ordinary fireplace with a good draught if a blower be used. Anomalous results are obtained if sufficient care be not paid to this point, as appears in the following paragraphs.

§ 15. *Experiment.*—A red-hot ball is hooked out of the fire with the insulating hook and presented to the + conductor of an electric machine in action, and then brought near to and into contact with an electroscope. No charge is imparted (compare § 1).

§ 16. *Experiment.*—The conditions being the same as in § 15, the − conductor of the machine is used; the electroscope leaves diverge permanently with − electricity on completing the contact.

We have in this experiment the first decisive evidence of the difference between the influences of hot iron on + and − electricity.

§ 17. *Experiment.*—As the red-hot iron ball on the insulating hook gradually cools, it slowly acquires the power of receiving from the machine and giving to the electroscope a charge of + electricity. So that, with regard to + electricity, white-hot and red-hot iron refuse to receive or convey a charge. With regard to − electricity, white-hot iron alone does so.

§ 18. *Experiment.*—A white-hot ball is hooked out of the fire on a conducting hook; and, as it cools, its power of inductively discharging + and − electricity is tested. Long after it has

lost all power of discharging the + electricity of the electroscope it still retains its power, only slightly diminished, of discharging − electricity. This experiment is perhaps the most striking one of this immediate series. The temperature at which the difference of behaviour of the iron towards the two electricities is most noticeable is a dull red heat, called, I believe, a cherry-red. Attempts are made in the sequel to approximately measure this crucial temperature.

§ 19. *Experiment.*—The white-hot ball is removed from the fire on the insulating hook, and its power of discharging + and − electricities frequently compared as it cools. As with the earth-connected ball of § 18, its power of discharging − endures long after its power of discharging + is lost.

§ 20. *Experiment.*—At a dull red heat, a ball, whether insulated or earth-connected, will not discharge a positively charged electroscope at a distance of half an inch.

§ 21. *Experiment.*—At a dull red heat a ball will discharge − electricity when earth-connected, but not when insulated. If the insulated ball which has, by cooling, just lost its power of discharging − electricity be held half an inch above the electroscope and momentarily touched with the finger or a wire, the leaves instantly but only partially collapse. At each touch a further collapse takes place.

§ 22. The whole aspect of the experiments with hot iron balls is of a nature to convince the actual experimenter that in all the above cases the collapse of the leaves is due to the escape of their prevalent electricity, and not to the accession of electricity of the opposite kind; for in no case do the leaves diverge after collapsing. A good many experiments of various kinds, which are sufficiently self-suggestive to release me from a description of them, showed that the leaves of a neutral electroscope were in no instance induced to diverge by the approach of hot bodies.

§ 23. We have so much greater control over the heat of an imperfectly conducting wire than over that of a hot metallic ball (we can retain the former constant, both in regard to its temperature and position, so much better than the latter), that I was soon induced to substitute the former for the latter. But as we know that galvanic currents passing along a wire exercise a locally wide range of galvanic influence, and that such influence is especially manifested when the current is started or stopped, approached or withdrawn from the influenced body, or increased or diminished in strength, it became necessary to examine whether under any conditions the current affected the electroscope when little heat was developed in its passage.

§ 24. *Experiment.*—The current from eight platin-zinc cells was passed along a copper wire $\frac{1}{8}$ inch thick and 9 inches long,

forming a circular loop above the cap of the electroscope, and about $\frac{1}{4}$ inch from it. No effect was produced, whether the electroscope was charged with + or − electricity.

§ 25. *Experiment.*—A current from eight platin-zinc cells was sent through a flat coil about 20 yards long, intermitting by means of an automatic "make-and-break" of an induction-coil. No effect was produced when the flat coil was within $\frac{1}{10}$ inch of the electroscope charged with + or − electricity.

§ 26. *Experiment.*—A secondary flat coil, closed and similar to that of § 25, being placed within $\frac{1}{10}$ inch of a charged electroscope, the intermittent charge was passed through the neighbouring primary; and no effect was produced on an electroscope charged with + or − electricity.

§ 27. The ground being thus cleared by the experiments of §§ 24, 25, and 26, which show that the passage of a cold galvanic current in the neighbourhood of a charged electroscope is without influence, and this being confirmed by the approach and withdrawal of the poles of a powerful permanent magnet, which are also without effect, the action of the heat developed in a platinum wire by means of the current could be studied.

§ 28. *Experiment.*—By means of eight platin-zinc cells a platinum wire of ·01 inch diameter and 4 inches long can be raised to whiteness. A +-charged electroscope is placed an inch below such a coil. About three seconds after the completion of the circuit the platinum wire becomes white-hot; and by this time the whole of the electricity is discharged. With − electricity the discharge is even more rapid, and is complete even before the wire is white-hot. At a distance of six inches the white-hot platinum-foil discharges both + and − electricity from the electroscope in about seven seconds. If the wire be near its fusing-point, there is little difference either as to distance or time in which the effect is produced with the two kinds of electricity.

§ 29. *Experiment.*—Nine inches of the platinum wire of § 28 were bent into a single horizontal circular loop, one inch above the electroscope which was charged with − electricity. The wire was heated to a just visible redness. The electroscope's leaves collapsed in two seconds, one of which is certainly spent in heating the wire, and the other is not more than is required for the fall of the leaves. It appears as though the fall of the leaves were instantaneous. A +-charged electroscope under like conditions requires six or seven seconds to be discharged.

§ 30. *Experiment.*—The loop of platinum wire arranged as in § 29 and heated to the same temperature, but placed at a distance of 5 inches above the electroscope, discharges − electricity in two seconds, but requires eight or ten seconds to discharge +.

§ 31. *Experiment.*—The 8-cell battery which I used was not sufficient to heat twelve inches of the wire when in a single loop even to dull redness. Such a non-luminous hot loop discharged an electroscope of − electricity, when at a distance of one inch, in seven or eight seconds. A + charge is not perceptibly affected.

§ 32. It appears, on comparing the experiments of §§ 1–21 with those of §§ 28–31, that the behaviour of red- and white-hot earth-connected iron balls is precisely analogous to the behaviour of red- and white-hot platinum wires heated by galvanism. In experiments §§ 28–31 the battery was not insulated from the ground. I may mention that repeated and varied attempts by insulating the battery to detect a difference in the discharging-power between an earth-connected and insulated galvanically heated wire (similar to the difference between the discharging-powers of earth-connected and insulated hot iron balls) failed, presumably on account of the considerable amount of neutral matter in the battery, which must be electrically connected with the wire.

§ 33. *Experiment.*—When the black bottom of a pan of boiling water is brought within $\frac{1}{4}$ inch of the top of an electroscope charged with + or − electricity, ordinary temporary inductive discharge takes place: the leaves recover their divergence completely on the removal of the pan. Yet the actual quantity of heat, as measured by air-expanding power radiated by the bottom of the pan upon the electroscope at the distance of $\frac{1}{4}$ inch, is far greater than that radiated upon it at a distance of 6 inches by the 4-inch long white-hot platinum wire. This is clearly enough shown by employing the differential air-thermometer with conical copper air-chambers blackened above. The pan has a base about 700 times as great as the orthographic projection of the outline of the spiral, and indefinitely greater than the projection of the wire itself. Immediately the spiral is heated white-hot above one cone, the pan of boiling water (containing an immersed "heater") is brought so near to the other that the liquid in the bent stem remains for a time sensibly at rest. The wire is 6 inches from its cone. The pan has to be placed at a distance of 8 inches to produce equilibrium; and therefore, if the mere quantity of radiant heat received by the electrified body were at all proportional to the amount of discharge, the pan at 8 inches should discharge the electroscope as rapidly and completely as the white-hot spiral at 6 inches. But, as before stated, its permanent discharging-power is practically nothing, even at $\frac{1}{4}$ inch. Hence it is clear that the discharging-power depends far more upon the *quality* of the heat radiated from the *discharger*, than upon the quantity of heat (as measured in heat-units) received by the electrified body.

§ 34. *Experiment.*—If a series of dielectric non-conductors, such as a series of plates of varnished glass, be placed on an electroscope which is then charged with — electricity, a dull red-hot ball or platinum wire a few inches above the electroscope causes the leaves to collapse, but does not discharge the system. On removing the plates in succession (after the removal of the heated discharger), the leaves diverge more and more, till the withdrawal of the last leaves them nearly fully charged. The bottom of each plate is found to be charged with + electricity. A similar effect is produced, changing the changeables, by a white-hot ball or wire above the upper of a series of varnished glass plates on the top of a positively charged electroscope.

§ 35. The experiments with electroscopes may be repeated almost one for one with the condensed electricity of a Leyden jar. Owing, I presume, to the closeness of the two condensed electricities to one another on the metallic coatings, and to that penetration of the two kinds into the glass which furnishes the potential residual charge, the discharge of a jar is never so rapid or complete under the same circumstances as that of an electroscope.

§ 36. *Experiment.*—A Leyden jar of about $1\frac{1}{2}$ square foot outer foil surface is fully charged with + electricity. A white-hot earth-connected iron ball, at a distance of 3 inches above the electroscope, discharges the jar, but by no means completely, in thirty seconds (when the ball had become dull red). A jar charged with — electricity is discharged under like conditions almost completely in thirty seconds. The remaining charge is not greater than the first residual charge under ordinary circumstances (the glass being about $\frac{1}{8}$ inch thick).

§ 37. A dull red-hot ball earth-connected at a distance of 2 inches from a +-electrified jar discharges scarcely any of the charge in a minute, while a — charge in the same time is considerably diminished.

§ 38. *Experiments.*—A platinum spiral of wire 4 inches long and ·01 inch thick is fixed 4 inches above a charged jar. The latter loses little of its charge in one minute. When the current passes through the wire from eight platin-zinc cells the wire becomes white-hot, and the charge, whether + or —, is lost in one minute. For the success of this experiment, especially when + electricity is dealt with, the wire must be almost at the point of fusion.

§ 39. *Experiment.*—A 9-inch single-loop wire, one inch from the jar, heated to incipient redness, discharges about half of the electricity of a +-charged jar and almost the whole of a —-charged one.

§ 40. Two electroscopes, charged respectively with + and —

electricities, have their caps connected by wires with two foil-covered balls standing on insulating stands. A loop-wire of platinum connected with a battery is equidistant from the balls and between them. Whatever the temperature of the wire may be, the leaves fall equally in the two electroscopes; that is, *both fall equally quickly with a white-hot wire, and equally more slowly with a red-hot wire.*

§ 41. *Experiment.*—By means of a plano-convex glass lens of 6-inch focus, the sun's direct rays may be concentrated so as to ignite paper; when the cap of an electroscope charged with + or − electricity is placed in this focus no sensible discharge ensues. A small blackened brass knob, connected with the electroscope and submitted to the heat of the sun similarly concentrated, does not cause the leaves to collapse. The Leyden jar has its knob polished or blackened; whether the jar be charged with + or − electricity, no discharge ensues when the sun's rays are concentrated upon it by a lens.

§ 42. *Experiment.*—The polished or blackened knob of a Leyden jar, or the blackened knob connected with an electroscope, is in the principal focus of a spherical metallic mirror 18 inches in diameter. In the focus of a conjugate mirror is placed a white-hot iron ball. The centres of the mirrors are 5 feet 9 inches apart, and their foci 3 feet 10 inches. Though in the course of $1\frac{1}{2}$ minute the blackened objects become too hot to hold in the hand, though paper is scorched in the receptive focus, neither + nor − electricity of a jar or electroscope is discharged.

§ 43. Experiments § 41 and § 42 show that a certain proximity must exist between the source of heat and the charged body in order that discharge may take place, and that the discharge is not due only to the intensity of the heat.

§ 44. *Experiment.*—The edge of the sun's image was made to touch the cap of an electroscope; and an earth-connected iron ball was, so to say, dipped into the image. Whether positively or negatively charged, the electroscope only showed the usual temporary inductive release of its electricity. This experiment, to which I attach very great importance in the series under consideration, seems to show, in conjunction with §§ 41 & 42, that for there to be discharge it must be the inducing body itself which is the source of heat, and that the mere presence of heat of passage geometrically filling the space between the electrified body and the inducing body is not sufficient for the discharge.

§ 45. *Experiments.*—A charged jar is placed on an insulating support of varnished glass. Its outer coating is connected with one terminal of an astatic galvanometer having a current-conducting wire of great length and thinness; the other terminal is

connected with the earth. Above the knob of the jar is the incandescible platinum wire. As the electricity of the inner coating (+ or −) is discharged, that of the outer coating (− or +) is released, and, traversing the wire of the galvanometer, deflects its needle. The effect may be increased by using a battery of jars as above, or by placing the discharging-wire between the knobs of oppositely electrified jars whose outer coatings are connected with the opposite terminals of the galvanometer. By such means more striking effects can be produced than by the uncondensed electricity of the conductor of the machine.

§ 46. The assertion that glass is a good conductor of electricity when red-hot at once suggests a possibility of connecting such a statement with the above experiments of discharge.

§ 47. *Experiment.*—Nevertheless this statement is true; for if the end of a glass tube be made red-hot, it may indeed be placed upon the top of a + or − charged electroscope (being held by the cool end) without discharging it; but if, when so resting, it be touched by a wire earth-connected, the charge is immediately and completely lost.

§ 48. *Experiment.*—The end of a stout glass tube is heated red-hot; on touching with the hot part either conductor of an electric machine, it is found that both + and − electricities may be abundantly conveyed from the conductor to the electroscope. This faculty continues with both electricities after the glass has ceased to be incandescent at all. It ceases with + before it ceases with −; indeed with the latter it is retained almost to a temperature as low as 100° C.

§ 49. *Experiment.*—In examining the discharging-power of red-hot glass some noteworthy facts are observed. When the red-hot glass is insulated, as when it forms the extremity of a long glass rod, there is a difference in the behaviour of + and − electricities. With + the hot glass merely acts as an inductive discharger; the leaves temporarily collapse, and return to their divergence when the glass is withdrawn. When the electroscope is charged with − electricity, the first approach of the hot glass causes partial collapse, as by simple induction. On withdrawing the glass, further collapse ensues; on bringing the glass again near, the same amount of divergence is produced which existed before withdrawal.

§ 50. On the 4th of January, 1873, at 3–4 P.M., I observed that the difference between the discharging-power for + and − electricity was greatly diminished and sometimes extinguished. There was at the time a severe storm over London, unaccompanied however, as far as I know, by electric atmospheric disturbance. On inquiry at the Observatories of Kew and Greenwich, Mr. Whipple informs me that, for a temporary reason, there is no

record of the atmospheric electricity on that day. The Astronomer Royal informs me that the atmosphere was + on the morning of that day, but that of the afternoon there is no record.

§ 51. The following attempts were made to measure approximately the temperature (1) at which a hot insulated iron ball commenced or ceased to discharge − electricity when at a distance of 2 inches from an electroscope, (2) at which an insulated iron ball begins or ceases to discharge + electricity at the same distance of 2 inches, (3) at which there is no sensible difference between the discharge of + and −. These experiments were checked by examining the temperatures at which an insulated iron ball on cooling (1) commences to be able to receive −, (2) commences to be able to receive +, and (3) freely and equally receives both.

§ 52. *Experiments.*—The heats of balls at the critical temperatures were taken by plunging them into weighed quantities of cold water, weighing them both together. Taking into account the weight and specific heat of glass, and calling the sum of the heat-units in one gramme of iron from 0° C. to the temperature t° C.,

$$\mathrm{Fe}\Sigma_0^t w,$$

it was found that

$$\begin{aligned} \mathrm{Fe}\Sigma_0^t w &= \left.\begin{matrix} 84\cdot7 \\ 83\cdot7 \end{matrix}\right\} \quad \ldots\ldots\ldots \quad (1) \\ &= \left.\begin{matrix} 115\cdot9 \\ 116\cdot3 \end{matrix}\right\} \quad \ldots\ldots\ldots \quad (2) \\ &= \left.\begin{matrix} 145\cdot1 \\ 137\cdot0 \end{matrix}\right\} \quad \ldots\ldots\ldots \quad (3) \end{aligned}$$

At (1) both electricities cease to be discharged, and the insulated iron can serve as a proof plane for both kinds. Between (1) and (2) − electricity is discharged only, and the insulated iron may serve as a carrier for − electricity, but not for +. At (3) and above, both electricities are discharged equally, and the iron refuses to receive a charge of either kind.

August 28, 1873.

XXXII. *On a new Mechanical Theorem relative to Stationary Motions.* By R. Clausius.

[Concluded from p. 244.]

7. THE above-illustrated notion of the phase, which refers to periodic changes in the motion, can be employed in the consideration of motions which take place simultaneously in closed paths. But when we have a system of points which, though moving in a stationary manner, do not describe closed

paths, and with which also the individual variables by which the positions of the points are determined do not simply change their values periodically, a somewhat more general conception must be employed, which can be comprehended as the phase in a wider signification of the term.

Employing again the quantities $q_1, q_2, \dots q_n$ for the determination of the positions of the points, without presupposing that every quantity repeats its variations regularly in periods of determined length, we shall yet introduce for each quantity a certain interval of time, which may be denoted by $i_1, i_2, \dots i_n$. With the aid of these, we will define the phases belonging to the different quantities, and which may be called $\phi_1, \phi_2, \dots \phi_n$, by the following equations:—

$$t = i_1\phi_1 = i_2\phi_2 \dots = i_n\phi_n. \quad . \quad . \quad . \quad . \quad (12)$$

Now let $q_1, q_2, \dots q_n$ be variated, and with each variable the phase belonging to it be regarded as the measuring quantity, which in the variation remains constant, while the time-interval may undergo an alteration. The variations so formed are, according to the above, to be represented by the symbols

$$\delta_{\phi_1} q_1, \ \delta_{\phi_2} q_2, \dots \delta_{\phi_n} q_n.$$

Employing such a variation, we will form for the variable q_ν, the fraction

$$\frac{p_\nu \delta_{\phi_\nu} q_\nu - h\delta k}{t}.$$

If the quantity q_ν accomplished its variations in a periodical manner, and i_ν were the duration of its period, the variation $\delta_{\phi_\nu} q_\nu$ would also alter only periodically, and accordingly the fraction, which has t in its denominator, would make continually smaller fluctuations and so approximate to zero. The same would hold for all the n variables if they changed in a periodical way, in which each might have its special period-duration. But now we will not make the definite assumption that the variations of the quantities $q_1, q_2, \dots q_n$ are periodic, but only fix the condition that the mean value of the sum

$$\Sigma \frac{p\delta_\phi q - h\delta k}{t}$$

for great times shall become very little—a condition which, according to the preceding, is at all events satisfied by periodic variations, but can also be fulfilled by other variations if they take place in a stationary manner.

After these preliminary remarks the following theorem can now be stated:—

If the variations, in the formation of which the quantities

$\phi_1, \phi_2, \ldots \phi_n$ *determined by the equations*

$$t = i_1\phi_1 = i_2\phi_2 \ldots = i_n \phi_n$$

can be regarded as constant, satisfy the condition that the sum

$$\frac{\Sigma p\delta_t q - h\delta k}{t}$$

has a mean value that vanishes as the time increases, then the following equation is valid:—

$$\delta(\overline{\mathrm{U}} - \overline{\mathrm{T}}) = \Sigma\overline{pq'}\delta \log i + \Sigma \frac{\overline{d\mathrm{U}}}{dc}\delta c, \quad . \quad . \quad . \quad \text{(II.)}$$

in which the first sum on the right-hand side, like the sum previously mentioned, contains n *terms, which correspond to the* n *variables* $q_1, q_2, \ldots q_n$; *while the second sum refers to the quantities* c_1, c_2, *&c. contained in* U, *which are constant during each motion, but change their values on the transition from the one motion to the other.*

Equation (II.) is my equation in the generalized form mentioned at the commencement. While in Hamilton's equation (I.) the integral $\int_0^t 2\mathrm{T}dt$ is to be regarded as a function of the variables $q_1, q_2, \ldots q_n$, of their initial values $k_1, k_2, \ldots k_n$, and of the energy E, and in equation (I. a) the integral $\int_0^t (\mathrm{T}-\mathrm{U})dt$ as a function of $q_1, q_2, \ldots q_n, k_1, k_2, \ldots k_n$, and t, in this equation the mean value $\overline{\mathrm{U}} - \overline{\mathrm{T}}$ appears as a function of the time-intervals $i_1, i_2, \ldots i_n$ and the quantities c_1, c_2, &c. It also can be analyzed into as many partial equations as there are independent variations on the right-hand side, whereby, however, we of course obtain quite different equations from those resulting from the analysis of Hamilton's equations.

8. In order to demonstrate the theorem, let us form for any one of the n variables the product $p\delta_t q$, and differentiate this according to the time. Thereby we obtain

$$\begin{aligned} \frac{d}{dt}(p\delta_t q) &= p\frac{d(\delta_t q)}{dt} + \frac{dp}{dt}\delta_t q \\ &= p\delta_t\left(\frac{dq}{dt}\right) + \frac{dp}{dt}\delta_t q \\ &= p\delta_t q' + \frac{dp}{dt}\delta_t q. \end{aligned}$$

Into this, for the abbreviated symbol p, we introduce from (6)

the fuller expression $\frac{dT}{dq'}$, and further, agreeably to equation (8), put

$$\frac{dp}{dt}=\frac{dT}{dq}-\frac{dU}{dq}.$$

Then follows

$$\frac{d}{dt}(p\delta_t q)=\frac{dT}{dq'}\delta_t q'+\frac{dT}{dq}\delta_t q-\frac{dU}{dq}\delta_t q. \quad . \quad . \quad . \quad (13)$$

An equation of this form holds for each of the n variables; and if we imagine the sum formed from these n equations, we obtain

$$\frac{d}{dt}\Sigma p\delta_t q=\Sigma\frac{dT}{dq'}\delta_t q'+\Sigma\frac{dT}{dq}\delta_t q-\Sigma\frac{dU}{dq}\delta_t q. \quad . \quad (14)$$

Since the quantity T is a function of the $2n$ quantities $q_1, q_2, \ldots q_n$ and $q'_1, q'_2, \ldots q'_n$, we can put

$$\delta_t T=\Sigma\frac{dT}{dq}\delta_t q+\Sigma\frac{dT}{dq'}\delta_t q',$$

an expression which contains the first two sums on the right-hand side of our preceding equation. As regards the last sum in that equation, if in U the quantities $q_1, q_2, \ldots q_n$ were the only variables, it could be replaced by $\delta_t U$. But as in U, by hypothesis, other quantities c_1, c_2, &c. occur, which, though independent of the time, yet may change their values on passing from one motion to the other, therefore

$$\delta_t U=\Sigma\frac{dU}{dq}\delta_t q+\Sigma\frac{dU}{dc}\delta c.$$

By the employment of these two equations, (14) is transformed into

$$\frac{d}{dt}\Sigma p\delta_t q=\delta_t T-\delta_t U+\Sigma\frac{dU}{dc}\delta c,$$

or, differently arranged,

$$\delta_t(U-T)=-\frac{d}{dt}\Sigma p\delta_t q+\Sigma\frac{dU}{dc}\delta c. \quad . \quad . \quad (15)$$

This equation, multiplied by dt, then integrated from 0 to t, and finally divided by t, since h and k are the initial values of p and q, takes the following form:—

$$\frac{1}{t}\int_0^t\delta_t(U-T)dt=-\Sigma\frac{p\delta_t q-h\delta k}{t}+\Sigma\frac{1}{t}\int_0^t\frac{dU}{dc}\delta c dt.$$

In the last term of the right-hand side, by making use of the

mark for mean values, we can put

$$\frac{1}{t}\int_0^t \frac{dU}{dc}\delta c dt = \overline{\frac{dU}{dc}}\,\delta c.$$

Meanwhile, on the left-hand side the integral-symbol may remain, and only the variation-symbol δ_t be transposed, which is admissible with variations where t is regarded as constant. Then the equation is

$$\delta_t\left[\frac{1}{t}\int_0^t (U-T)dt\right] = -\Sigma\frac{p\delta_t q - h\delta k}{t} + \Sigma\overline{\frac{dU}{dc}}\,\delta c. \quad . \quad (16)$$

Here, instead of variations in which the time is regarded as constant, we will introduce on the right-hand side variations in which the *phases* belonging to the variables in question are considered constant.

The procedure to be employed in this alteration is easily found, as follows. Let any quantity dependent on the time be signified by D, we will put, with the original motion,

$$Z = F(t),$$

and, with the different motion,

$$Z^* = F(t^*) + \epsilon F_1(t^*),$$

in which t and t^* represent times corresponding to one another, F and F_1 signify any two functions, and ϵ is an infinitesimal constant factor. If now the variation $\delta_t Z$ is to be taken, we have simply to put $t^* = t$ and then to form the difference $Z^* - Z$, whereby we obtain

$$\delta_t Z = \epsilon F_1(t);$$

while if the variation $\delta_\phi Z$ is to be taken, for t^* we must put that value of the time which corresponds to an unvaried value of ϕ, namely

$$t^* = t + \delta_\phi t,$$

and then again form the difference $Z^* - Z$. We have thus

$$\delta_\phi Z = F(t + \delta_\phi t) + \epsilon F_1(t + \delta_\phi t) - F(t).$$

This gives, neglecting terms of higher order with respect to $\delta_\phi t$ and ϵ,

$$\delta_\phi Z = \epsilon F_1(t) + \frac{dF(t)}{dt}\delta_\phi t,$$

which, according to the preceding, we can also write thus,

$$\delta_\phi Z = \delta_t Z + Z'\delta_\phi t. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

An equation of this form is to be constructed for each of the variables $q_1, q_2, \ldots q_n$, wherein the phases $\phi_1, \phi_2, \ldots \phi_n$ are to be successively employed. We thus obtain for q_ν, with a little

transposition of the terms, the equation

$$\delta_t q_\nu = \delta_{\phi_\nu} q_\nu - q'_\nu \delta_{\phi_\nu} t.$$

By the insertion of these values equation (16) is changed into

$$\delta_t\left[\frac{1}{t}\int_0^t (\mathrm{U}-\mathrm{T})\,dt\right] = \Sigma pq'\frac{\delta_\phi t}{t} - \Sigma\frac{p\delta_\phi q - h\delta k}{t} + \Sigma\frac{\overline{d\mathrm{U}}}{dc}\delta c. \quad (18)$$

Putting herein, from (12),

$$t = i_\nu \phi_\nu,$$

whence follows

$$\delta_{\phi_\nu} t = \phi_\nu \delta i_\nu,$$

and

$$\frac{\delta_{\phi_\nu} t}{t} = \frac{\delta i_\nu}{i_\nu} = \delta \log i_\nu,$$

we get

$$\delta_t\left[\frac{1}{t}\int_0^t (\mathrm{U}-\mathrm{T})\,dt\right] = \Sigma pq'\delta\log i - \Sigma\frac{p\delta_\phi q - h\delta k}{t} + \Sigma\frac{\overline{d\mathrm{U}}}{dc}\delta c. \quad (19)$$

In this equation, which holds for any time whatever, we will now take the mean values of all the terms. The last sum (which, indeed is independent of the time) is thereby unchanged. The mean value of the penultimate sum is, by hypothesis, for large times, to be put $=0$. In the rest of the terms we will only indicate the mean values. We thus obtain

$$\overline{\delta_t\left[\frac{1}{t}\int_0^t (\mathrm{U}-\mathrm{T})\,dt\right]} = \Sigma\overline{pq'}\delta\log i + \Sigma\frac{\overline{d\mathrm{U}}}{dc}\delta c. \quad . \quad (20)$$

In this we have still to consider more closely the left-hand side. The expression

$$\frac{1}{t}\int_0^t (\mathrm{U}-\mathrm{T})\,dt$$

is the mean value of $\mathrm{U}-\mathrm{T}$ during the time from 0 to t, and therefore a function of t which with the increase of t comes ever nearer to the constant value $\overline{\mathrm{U}}-\overline{\mathrm{T}}$, which represents the mean value for very large times. It does not, however, follow that the variation of this function, denoted by δ_t, must also approximate to a fixed boundary value. We have previously seen that, with a function whose changes consist only of fluctuations of constantly equal magnitude, the variation denoted by δ_t may take ever greater values with increasing time. Corresponding to this, with a function of the kind now in question, which with increasing time makes ever smaller fluctuations, and thus approaches towards a boundary value, it must be considered possible that the variation denoted by δ_t makes fluctuations the magnitude of which does not diminish as the time increases. It would

hence not be universally admissible to replace the variation

$$\delta_t\left[\frac{1}{t}\int_0^t(\mathrm{U}-\mathrm{T})\,dt\right]$$

by the symbol

$$\delta(\overline{\mathrm{U}}-\overline{\mathrm{T}}),$$

which represents the variation obtained when the mean value $\overline{\mathrm{U}}-\overline{\mathrm{T}}$ is regarded as a quantity independent of the time and is variated.

In our equation (20), however, the first of the two variations just mentioned does not itself occur, but only its *mean value.* This becomes constant for large times, as can indeed be seen from this, that an expression which becomes constant for large times stands on the right-hand side of the equation. Consequently the previously mentioned distinction, based on the variability of the variation, ceases; and we can therefore employ the symbol $\delta(\overline{\mathrm{U}}-\overline{\mathrm{T}})$ for this *constant-becoming mean value of the variation.* Thereby equation (20) is changed into

$$\delta(\overline{\mathrm{U}}-\overline{\mathrm{T}})=\Sigma\overline{pq}\,\delta\log i+\Sigma\overline{\frac{d\mathrm{U}}{dc}}\,\delta c,$$

or the equation (II.) which was to be demonstrated.

9. As an example of the application of the equation, we will take a simple special case for closer consideration.

Given two material points which, according to any law, attract and, at certain distances, repel each other, and under the influence of this force move about one another.

As the centre of gravity of the system remains fixed, and the motion of both points takes place in one plane, we can determine their position by two variables—their mutual distance r, and the angle θ which the line that joins them makes with a fixed straight line. If, namely, the masses of the two points be denoted by m and μ, their distances from their common centre of gravity are

$$\frac{\mu}{m+\mu}r \text{ and } \frac{m}{m+\mu}r.$$

If, further, by θ be understood specially the angle which the part of the right line r from the centre of gravity to the mass m makes with the positive x-direction of a rectangular-coordinate system taken in the plane of motion, the rectangular coordinates of the two points can be expressed as follows:—

$$x_1=\frac{\mu}{m+\mu}r\cos\theta,\qquad y_1=\frac{\mu}{m+\mu}r\sin\theta,$$

$$x_2=-\frac{m}{m+\mu}r\cos\theta,\quad y_2=-\frac{m}{m+\mu}r\sin\theta.$$

With the aid of these expressions the equation

$$T=\frac{m}{2}(x_1'^2+y_1'^2)+\frac{\mu}{2}(x_2'^2+y_2'^2)$$

can be transformed into the following:—

$$T=\frac{1}{2}\frac{m\mu}{m+\mu}(r'^2+r^2\theta'^2). \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Putting now r and θ in the place of the variables universally denoted above by q_1 and q_2, we obtain

$$\left.\begin{aligned} p_1&=\frac{dT}{dr'}=\frac{m\mu}{m+\mu}r', \\ p_2&=\frac{dT}{d\theta'}=\frac{m\mu}{m+\mu}r^2\theta'. \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad (22)$$

From this results further, if R, R′, Θ, Θ′ denote the initial values of r, r', θ, θ', the equation

$$\Sigma\frac{p\delta_\phi q-h\delta k}{t}=\frac{m\mu}{m+\mu}\,\frac{r'\delta_{\phi_1}r-R'\delta R+r^2\theta'\delta_{\phi_2}\theta-R^2\Theta'\delta\Theta}{t}. \quad (23)$$

For the definition of the phases ϕ_1 and ϕ_2 we have, according to (12), the equations

$$t=i_1\phi_1=i_2\phi_2; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

and the question now is, whether the time-intervals i_1 and i_2 can be so determined that the mean value of the expression given in (23) shall vanish with increasing time. With even a superficial consideration of the motion in question, it is seen at once which time-intervals are to be selected as i_1 and i_2, because the motion can be analyzed into two constituents—the alternate approach and recession of the two points, and the rotation of their connecting line—which can be considered singly, as variations of the quantities r and θ.

The variation of r is periodic; and if we take the duration of its period as i_1, the portion of the fraction in (23) relating to r, viz.

$$\frac{r'\delta_{\phi_1}r-R'\delta R}{t},$$

the numerator of which changes only periodically, evidently fulfils the condition that its mean value vanishes with increasing time.

For the interval which refers to θ it will be convenient to take the time of a rotation of the connecting line, therefore the time in which the angle θ increases to 2π. But as the successive rotations do not generally take place in equal times, we will un-

derstand by i_2 the *mean* rotation-period of the connecting line. For the mean angular velocity $\overline{\theta'}$ we accordingly obtain the equation

$$\overline{\theta'}=\frac{2\pi}{i_2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Further, on account of the proposition that the radii vectores of the points describe equal areas in equal times, we have the equation

$$r^2\theta'=a, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

where a is a constant; and we can therefore put

$$\theta'=a\frac{1}{r^2} \text{ and } \overline{\theta'}=a\overline{\frac{1}{r^2}}.$$

By employing these equations the identical equation

$$\theta'=\overline{\theta'}+\theta'-\overline{\theta'}$$

can be brought into the following form,

$$\theta=\frac{2\pi}{i_2}+a\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right);$$

and multiplying this equation by dt, and integrating from 0 to t, we get

$$\theta=\Theta\frac{2\pi}{i_2}t+a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt,$$

which, on account of $t=i_2\phi_2$ passes into

$$\theta=\Theta+2\pi\phi_2+a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt. \quad . \quad . \quad . \quad (27)$$

This expression for θ may now be variated, ϕ_2 being regarded as constant; we thereby obtain

$$\delta_{\phi_2}\theta=\delta\Theta+\delta_{\phi_2}\left[a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt\right]. \quad . \quad . \quad . \quad (28)$$

The expression

$$a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt$$

is a function of t which changes periodically; and its periods have the same duration as the quantity r, namely i_1. We must therefore endeavour to introduce for the variation of this expression denoted by δ_{ϕ_2} that variation which is denoted by δ_{ϕ_1}.

According to equation (17), for any function Z we can put

$$\delta_{\phi_1}Z=\delta_t Z+Z'\delta_{\phi_1}t,$$

$$\delta_{\phi_2}Z=\delta_t Z+Z'\delta_{\phi_2}t,$$

from which it follows that

$$\delta_{\phi_2}Z=\delta_{\phi_1}Z+Z'(\delta_{\phi_2}t-\delta_{\phi_1}t).$$

As we have, further,

$$\delta_{\phi_1}t=\phi_1\delta i_1=t\frac{\delta i_1}{i_1},$$

$$\delta_{\phi_2}t=\phi_2\delta i_2=t\frac{\delta i_2}{i_2},$$

the preceding equation changes into

$$\delta_{\phi_2}Z=\delta_{\phi_1}Z+\left(\frac{\delta i_2}{i_2}-\frac{\delta i_1}{i_1}\right)tZ'. \quad . \quad . \quad . \quad . \quad (29)$$

If we apply the kind of transformation hereby exemplified to the last term of equation (28), we get

$$\delta_{\phi_2}\theta=\delta\Theta+\delta_{\phi_1}\left[a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt\right]+\left(\frac{\delta i_2}{i_2}-\frac{\delta i_1}{i_1}\right)ta\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right). \quad (30)$$

If now we consider the part of the fraction in (23) which refers to θ, we can give it a simplified form, since according to (26)

$$\frac{r^2\theta'\delta_{\phi_2}\theta-R^2\Theta'\delta\Theta}{t}=\frac{a(\delta_{\phi_2}\theta-\delta\Theta)}{t}$$

may be put; and when we introduce into this the preceding expression for $\delta_{\phi_2}\theta$, we obtain

$$\frac{r^2\theta'\delta_{\phi_2}\theta-R^2\Theta'\delta\Theta}{t}$$

$$=\frac{a}{t}\delta_{\phi_1}\left[a\int_0^t\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right)dt\right]+a^2\left(\frac{\delta i_2}{i_2}-\frac{\delta i_1}{i_1}\right)\left(\frac{1}{r^2}-\overline{\frac{1}{r^2}}\right). \quad (31)$$

The first term on the right-hand side of these equations makes with increasing time ever smaller fluctuations and thus approximates to zero; while the second term makes fluctuations of constantly equal magnitude. If, however, we take the *mean value* of the expression, the second term also vanishes, because the difference $\frac{1}{r^2}-\overline{\frac{1}{r^2}}$ is changed into $\overline{\frac{1}{r^2}}-\overline{\frac{1}{r^2}}$. Therefore the part of the fraction relative to θ, as well as the part relative to r, fulfils the condition laid down in our theorem, that its mean value vanishes with increasing time.

This having been established, we can apply to the present case the equation (II.) given in the theorem, and obtain thereby the following—

$$\delta(\overline{U}-\overline{T})=\frac{m\mu}{m+\mu}(\overline{r'^2}\delta\log i_1+\overline{r^2\theta'^2}\delta\log i_2)+\Sigma\ \overline{\frac{dU}{dc}}\delta c, \quad . \quad (32)$$

which exhibits a peculiar relation between the time-intervals i_1 and i_2 and the mean values of the ergal and *vis viva*.

If the mass μ be taken as very great in comparison with m, so that the fraction $\frac{m\mu}{m+\mu}$ may be supposed equal to m, the preceding equation changes into that which holds for the motion of one material point about a fixed centre. This equation I derived separately in a memoir published a short time since*; and I then said that for two points moving about each other the corresponding equation could in like manner be derived. Here, however, the same equation has come out as a special case of a much more general one.

To equation (II.) various other forms can be given, which are both theoretically interesting and convenient for use, wherein it can at the same time be brought into connexion with my theorem of the virial. These transformations and especially the application of the equation to the theory of heat I reserve for a subsequent memoir.

XXXIII. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* By James D. Dana.

[Continued from p. 219.]

IV. *Igneous Ejections, Volcanoes*†.

THE direct connexion of igneous ejections with the movements resulting from the earth's contraction has been briefly illustrated in the course of the remarks on mountain-making‡. It is apparent, without further discussion, that regions of great disturbances embrace the regions of great igneous ejections—that the oceanic slopes of the border mountains, especially around the Pacific, the greater ocean, have been preeminently subject to such eruptions—and that in Tertiary times, when the earth's crust was becoming too stiff to bend much before the lateral pressure, profound fractures instead of flexures were a common result of its action, igneous outflows were most extensive, and volcanoes were of increased size and numbers.

The questions remaining are:—

(1) Does the mobile rock owe its mobility in all cases, or any, to the movements of the rocks under lateral pressure—motion

* *Nachr. der K. Gesellsch. der Wiss. zu Göttingen*, Dec. 25, 1872; Phil. Mag. July 1873.

† For Part I. see p. 41; Part II. p. 210; Part III. p. 217.

‡ *Suprà*, pp. 52, 137, 212, 214.

transformed into heat being its source, in conformity with the views ably set forth by Mallet?

(2) Is the ejecting force in eruptions a result of the same lateral pressure?

1. *Source of igneous fusion**.—The suggestion of Professor Hopkins with regard to the earth's interior, which has been sustained on a preceding page†, offers for the source of igneous ejections the old one of viscous rock underneath the earth's crust; but viscous rock at first as a layer, and later as remnants of the same layer, left after the long cooling. This author suggests that these isolated or detached portions may be the source of modern volcanic eruptions; and this view is favoured by Scrope, the eminent vulcanologist‡.

The theory does not require that the isolated fire-lakes should exist now at the depth of the original viscous layer; for in the

* I say nothing here on the nature or the degree of igneous fusion in plastic rock, as this is foreign to the subject under discussion.

† Professor Hopkins's argument from the amount of precession and nutation, in the Transactions of the Royal Society, 1839, 1840, and 1842, led him to the conclusion that "the thickness of the solid shell could not be less than about one fourth or one fifth of the radius of its external surface." In his paper in the Report of the British Association for 1847 he repeats his conclusion, and then considers the possible steps in the process of refrigeration. Among the different conditions discussed, he supposes as one (the one he favours) the temperature of fusion to be increased much by the pressure—at a rate much more rapid than "1° F. for an increase of pressure equivalent to about five atmospheres," and adds that then "we should probably have the condition under which solidification would commence at the centre. In this case, after incrustation had begun at the surface, the earth would consist of a solid central nucleus and a solid shell with fluid matter between them, as already explained, till the solidification should be complete." He then remarks, with regard to the present state of our globe, that "under the condition here assumed, with the observed rate of increase of terrestrial temperature in descending, there could be no reasonable doubt of the earth's entire solidity." He next treats more in detail of the process of solidification, and says, having in view the several conditions discussed, that "the incrustation may have constituted the very first step in the solidification, or it may have taken place after the formation of a solid nucleus surrounded by a superficial fluid envelope, according as the solidification began at the surface or centre; but in either case, as I have already intimated, the superficial crust when first formed must necessarily have reposed on a fluid mass beneath." He next explains his views as to the formation of the crust, and the final obliteration of the viscous layer; and states, with reference to volcanoes:—"By a continuation of this process it is obvious that the superficial crust of our globe must at length arrive at what I conceive, for reasons above assigned [in a paragraph on p. 33, speaking of the isolated sources of volcanoes], to be its present condition,—that of a solid mass containing numerous cavities filled with fluid incandescent matter, and either entirely insulated or perhaps communicating in some cases by obstructed channels." These cavities are made the sources of the material of volcanic eruptions.

‡ Volcanoes, 2nd edit. 8vo, pp. 264, 268.

movements and flexures of the crust, and the opening of fissures for the escape of the mobile rock, great regions of it may have been pressed up and become isolated fire-lakes at a higher level. Or, as Professor Hopkins suggests, the subterranean sources of volcanic action may have been in shape like inverted cones extending far up into the crust, and may have finally been divided off into "smaller ones by a process similar to that by which the larger cavities were themselves formed from the general fluid mass, the position of each minor cavity being determined by the points in the roof of the larger one, where previous fractures had left the most perfect volcanic vents." This last explanation holds if regions of volcanic vents date back to early geological time, so that modern volcanoes in an area are successors to indefinitely older ones, but is less applicable where the vents of a region are of Cænozoic origin (as appears to be true of most of them) and a consequence of the later movements of lateral pressure.

The inference that igneous eruptions have generally been derived from a deep-seated source is sustained by the great lithological uniformity of ejections over widely distant ranges of surface. The eruptive rocks (or trap) of the Triassico-Jurassic areas of the Atlantic border from Nova Scotia to the Carolinas, already many times referred to*, all which belong to one epoch, are solely varieties of dolerite—rocks made up essentially of Labradorite and pyroxene, with more or less of magnetic iron-

* These hills and dikes of "trap" (as they are ordinarily called) have great length and breadth in Nova Scotia. In the Connecticut valley they are very numerous, and in many lines—as laid down, especially for Connecticut, by Percival in his 'Geological Report.' A copy of a part of his very accurate detailed map of the trap-region is reproduced in the writer's 'Manual of Geology,' page 20. At the east and west bend in the dikes, south of the middle of the map, are the Hanging Hills of Meriden, 900 to 1000 feet high, situated about 20 miles north of New Haven (and 12 miles from the southern margin of the map); and the range which extends from these hills north continues to Mount Tom, near Northampton, in Massachusetts. The range to the east, south of this bend, continues southward to Lake Saltonstall, east of New Haven. Other lines cut through the metamorphic rocks; one of these lines of trap-ejections in the eastern metamorphic region of Connecticut, as mapped by Percival, extends from Long-Island Sound, 6 miles east of New Haven, nearly to the southern boundary of Massachusetts, over 70 miles, with a nearly east-north-east trend, consisting of an interrupted series of dikes, intersecting metamorphic rocks of various kinds. A second independent Triassico-Jurassic trough exists in Connecticut, 15 to 20 miles west of the main one, over part of the towns of Southbury and Woodbury; and this also has its numerous trap dikes. The Palisades of New Jersey, bordering the Hudson River, are the northern part of a complex series that continues southward and westward through New Jersey and Pennsylvania into Virginia, following the course of the Triassico-Jurassic sandstone areas. There is also a series in the North-Carolina area.

ore in disseminated grains or crystals. Analyses have shown that the trap of this region in Connecticut and New Jersey is essentially identical in constitution; and an examination and determination of the density of specimens of the same rock from North Carolina and Nova Scotia leaves no doubt that it is fundamentally alike throughout. The aspect of the ordinary specimens from these widely distant regions is closely the same; and the density varies little from 3. The dolerite of East Rock, New Haven, Conn., gave Professor G. J. Brush for the specific gravity 3·09–3·085; from West Rock, ib., 3·045–3·07; from the vicinity of Raleigh, North Carolina (a specimen received by the writer from Professor Kerr, of Raleigh, and not distinguishable by the eye from an East or West Rock specimen), 3·16. Professor Cook, of New Jersey, obtained for the dolerite of the Palisades (Geol. Rep. N. J. p. 215) 2·94. Professor H. How gives the writer, for a similar variety from Nova Scotia, 2·94. Complete analyses have not been made of many of these rocks. The following are analyses of the West-Rock (New Haven) dolerite and that of the Palisades, New Jersey (seventy miles west of West Rock)—the former by W. G. Mixter, Assistant Chemist in the Sheffield Scientific School, and the latter by Professor G. H. Cook, of the New Jersey Geological Survey:—

	SiO^2.	$2AlO^3$.	FeO.	MgO.	CaO.	KO, NaO.	Ign.	
1. West Rock.	52·37	12·31	13·13	6·03	10·74	2·62	0·94	= 98·14
2. Palisades .	53·9	17·5	8·0	10·3	8·0	[2·3]	..	= 100

There are variations in the dolerite of the regions above referred to depending on the proportion of the felspar and also on the presence of water; but they occur in various parts of the several regions instead of characterizing any one of them. The dolerite of dikes in East Haven, Conn. (the town adjoining New Haven on the east), diminishes in lustre as the dike is more remote from the New-Haven line; and in the ridges near Saltonstall Lake, two miles distant, it is faintly glistening and of somewhat less hardness; and although generally solid throughout, it is in some parts vesicular (amygdaloidal). A precisely similar rock in all its characters occurs among the trap-hills (dolerite) of Meriden, called the Hanging Hills; and part of it is amygdaloidal. The same is found also in Nova Scotia. An examination of a specimen from the vicinity of Saltonstall Lake, by Professor O. D. Allen, shows the presence of 4·53 per cent. of matter driven off on ignition, after deducting the hygroscopic moisture (0·60). It indicates the presence of some hydrous mineral (chlorite apparently) in place of part of the augite or felspar; but the percentage of silica is 49·88, 50·10, according to two determinations by S. T. Tyson, of the Sheffield Scientific

School Laboratory, and thus differs but little from that of the anhydrous dolerite of West Rock and the Palisades; the proportion is less instead of greater. There is hence a close similarity to the other dolerite, notwithstanding the presence of water.

Since the dikes of this hydrated dolerite occur as parts of the same series as the others and in the same regions, and are of the same epoch and system of ejections, and moreover are exceptional instead of the prevailing kind, it is apparent that the hydrous character is owing to some exceptional condition attending or subsequent to the eruption. The source of liquid rock was all one; and the material must therefore have been essentially one in kind; it could not have been locally hydrous. Portions of the erupted viscid material might have encountered water on the way to the surface and thus have become penetrated with it*; so that the viscid material at the high temperature became altered in part—some of its augite to chlorite, and perhaps some of its felspar, with the lime of the augite, to zeolites, &c. Or else this kind of change may have taken place through infiltrating waters. The fact that the *vesicular* trap (now amygdaloidal) is that which is hydrated renders it rather probable that while zeolites and calcite may have been later made within the rock by superfical action or infiltrating waters, the chief change was contemporaneous with the eruption; for the vesicular character was undoubtedly then produced, and it was probably dependent on the same moisture that, penetrating the rock, caused the metamorphism or hydration through the mass. Bunsen, in his well-known paper on the igneous rocks of Iceland†, gives various

* On this taking-in of moisture by liquid rock, Mallet, speaking of the lava in the conduit of a crater, makes the following remarks:—"Within a few years it has been proved that capillary infiltration goes on in all porous rocks to enormous depths, and that the capillary passages in such media, though giving free vent to water (and the more as the water is warmer), are, when once filled with liquid, proof against the return through them of gases or vapours. So that the deeply seated walls of the ducts leading to the crater, if of such material, may be red-hot and yet continue to pass water from every pore (like the walls of a well in chalk), which is flushed off into steam that cannot return by the way the water came down, and must reach the surface again, if at all, by the duct and crater, overcoming in its way whatever obstructions they may be filled with. And this remarkable property of capillarity sufficiently shows how the lava (fused below or even at or above the level of infiltration) may become interpenetrated throughout its mass by steam-bubbles, as it usually, but not invariably, is found to be."

† Taylor's Scientific Memoirs, New Series, Part I. p. 33. The secondary origin of the zeolites and chlorite found to penetrate some dolerite and other kinds of igneous rock, as well as filling the cavities within them, I sustained in an article in vol. xlix. of the first series of Silliman's Journal (1845); and the various facts I have since observed have tended to confirm

facts illustrating this kind of metamorphism yielding chloritic and zeolitic minerals.

There is hence nothing in the relations of the different kinds of dolerite in the Connecticut valley or any part of the Triassico-Jurassic regions of the Atlantic slope, either as to geographical position or chemical composition, that favours the idea of difference of original subterranean source.

Now such a similarity of product occurring at intervals over a region 1200 miles long, and in several parallel lines, shows some unity of origin. It evinces that the source must have been either the undercrust fire-sea, like the under-Appalachian as we have termed it, derived from the old viscous layer, or else plastic masses within the true crust, which crust, as it was made from the viscous layer by cooling, would have like uniformity of mineral constitution.

This example of igneous eruption is grand in scale, since the outflows are from tappings along a line more than a thousand miles long; and it may therefore serve well to test the general application of theories of igneous eruptions. It indicates that the original fire-sea was one of great extent.

Professor T. Sterry Hunt, in his paper "On the probable Seat of Volcanic Action"*, and others of earlier date, while adopting the view of a nucleus made solid by pressure and of an exterior crust, with a viscid layer between, has proposed to substitute for the viscous layer recognized as the source of volcanoes by Hopkins, one made by the "aqueo-igneous fusion" of part of the lower sedimentary beds of the supercrust†. The alleged causes of such fusion are:—(1) pressure due to the accumulation of superficial sedimentary beds; (2) increase of heat, by a rise of the isogeothermals—another effect of the same cause; and (3) the presence of moisture, a common feature of sediments—the moisture becoming, under the heating, superheated vapour, and so aiding by its great dissolving power in the result. In a later article‡ he says, "I have taken pains [in former papers] to explain that the deeply buried layers of sediment, together with the superficial and water-impregnated portions of the solid nucleus, constitute a softened or plastic zone from which all

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me in the conclusion. In the paper referred to I attribute the change to infiltrating waters. The opinion has many advocates.

* Silliman's Journal, S. 2. vol. l. p. 21 (1870).

† I use the term supercrust as it is explained on page 216—that is, for the part of the crust made by the action of exterior agencies over the outer surface of the true crust, the true crust being the part that was formed directly from cooling.

‡ Geological Magazine, February 1868.

plutonic and volcanic rocks proceed, and which allows of the movements observed in the solid crust."

It will be perceived that the hypothetical viscous layer of Hunt lies between a rind made out of sedimentary formations and a solid nucleus which is at surface of sedimentary origin; and the viscous layer which Professor Hopkins regards as belonging to a stage in the process of solidification is supposed to have been closed up, or some way put *hors de combat*.

This substitution appears to be quite unnecessary, and the process incapable of producing the result claimed. As has been already explained*, the pressure from the gravitation of sediments cannot produce the heat needed for the fusion, any more than it can cause the plications alleged to attend it. If, then, no fusion of sediments would have resulted from such a cause, there was no chance for the formation of the deep and extended plastic zone required to meet the demands of the grand system of oscillations the earth's surface has experienced. In fact the conditions Professor Hunt's hypothesis appeals to (that is, thick sedimentary accumulations, such as those of the Appalachian region) are far too local for the production of so vast a plastic zone, even if fusion were a possible result of the accumulation.

Again, the facts mentioned on the preceding page give as positive a declaration against this origin of the material of igneous ejections through the fusion of sediments. The sedimentary strata along the Atlantic border, whether unaltered or metamorphic, deep-seated or superficial, vary every score of miles, and could not yield a uniform quality of fused material for the ejections. In the Connecticut valley the metamorphic rocks from New Haven, Conn., to Northern Massachusetts, a distance of a hundred miles, have *an east-north-east strike obliquely across the region of trap-ejections*, and are on the west side chloritic mica-slate, diabase, chlorite slate, argillite, mica-schist, gneiss, and other micaceous rocks of various kinds; and the Archæan (Azoic) rocks to the west in Dutchess Co., N. Y., and in Connecticut west of Winsted, are diverse varieties of gneiss and granitoid gneiss, containing orthoclase and albite or oligoclase with quartz, and sometimes hornblendic, but, as far as known, without Labradorite. Only wide diversity, not uniformity, could come from such varied material. Dolerite is, moreover, the last rock to be looked for from any of it, except it be from the diabase. For it has a low percentage of silica (46 to 52 per cent.) and no free quartz, with 8 to 12 per cent. of oxide of iron, about as much of lime and also of magnesia

* *Suprà*, page 217.

with very little potash or soda; and in these constituents there is comparatively little variation.

Hunt claims that the presence of moisture in many igneous rocks sustains the idea of derivation from the fusion of sediments —that is, from the fusion of rocks containing water. But it has been shown (on page 280) that the cases of hydrated dolerite in the region referred to are local and exceptional, and that the ridges are so mixed up with those of the more common anhydrous dolerite that all must have had one source. His argument would be of some weight if there were no source for the moisture except in the region of fusion. The hydrated rock, moreover, is shown to have the low percentage of silica which belongs to the anhydrous dolerite.

If subterranean fusion were somehow produced and a zone of mobile rock were thus made many scores of miles deep, with the material not simply plastic but of perfect liquidity, and the zone without obstructions in any part, so that there could be tidal movements or at least free-flowing currents throughout it, then, if the material entered into fusion were as diverse as the rocks of the Atlantic border, there might be such a mixing up and chemical digestion of limestone and every thing else, that the product would perhaps be doleritic—a rock low in silica (or basic). But these conditions are those of the earth's liquid envelope before and after refrigeration began, and of the viscous layer developed out of it, even probably to the basic character of the fused material; and further, they are what Hunt's method has no provision whatever for producing.

The facts afforded by other parts of the globe and other periods in its history add force to this argument. A doleritic or basaltic character is the prevailing one among the igneous eruptions of all ages from Archæan time to the present, and of all continents and oceans. Whitney and Pumpelly have described basic or doleritic rocks of Lower Silurian age from the Lake-Superior region, where they are the prevailing igneous rocks. Hunt has described and analyzed others, from Canadian localities, of the Laurentian, the Lower Silurian era, and of later Palæozoic time. They are far the most abundant of the igneous products of the Tertiary and Quaternary over the Pacific slope. Similar facts might be cited from other continents*. More than three fourths of true igneous rocks are probably of this nature. Hence it is reasonable to pronounce such ejections

* For comparison with the analyses on page 279, I add here others of the dolerite (trap) of (I.) the Giant's Causeway, (II.) Staffa, and (III.) Färöe, by Streng (Pogg. *Ann.* 1853, vol. xc. pp. 110, 114—all of which are now supposed to be of Tertiary origin,—and another (IV.) of an Archæan (Lau-

manifestoes of some kind of community of origin in the earth's ejections, as well as announcements of the kind of material that constitutes to a large degree the deeper regions of the crust.

There is, however, considerable diversity among true igneous rocks. Besides the prevalent doleritic iron-bearing kinds, including dolerite, peridotite, melaphyre, &c., to which the preceding observations relate, there are trachytic or felspathic rocks, whose constitution, especially in the case of the more siliceous varieties, comports with their origin from the fusion of granitic rocks or of sedimentary beds made of granitic material. They occur in the same regions in which doleritic dikes exist, as well as among volcanic products.

Such a composition does not, however, make it certain that the supercrust was their source; for true granite and gneiss and even quartzites occur constituting old Archæan terrains; and it may be that essentially the same kinds of rock are to some extent represented in regions of the infra-Archæan crust and even in the fire-seas.

Basaltic and trachytic rocks often occur so combined in a single volcanic mountain, that we seem forced to find some other explanation of their origin than that of the fusion of *unlike* sedimentary strata. As long since observed by Von Buch at

rentian) dolerite from Grenville, Canada, by Hunt (Geol. Rep. Canada, 1863):—

	I. G.=2·88.	II. G.=2·96.	III.	IV.
Silica	52·13	47·80	49·40	50·35
Alumina	14·87	14·80	14·62	17·35
Sesquioxide of iron	..	..	..	12·50[1]
Protoxide of iron	11·40	13·08	16·27	
Protoxide of manganese	0·32	0·09		
Magnesia	6·46	6·84	5·86	4·93
Lime	10·56	12·89	10·34	10·19
Soda	2·60	2·48	2·28	2·28
Potash	0·69	0·86	0·34	0·69
Volatile (H^2O &c.)	1·19	1·41	2·41	0·75
	100·22	100·25	101·52	99·04

Lyell recognizes the occurrence of "trap" in all periods from the Laurentian to the present (Principles, chap. vii.); and Mr. S. Allport announces in a recent article (Geol. Mag. May 1873), entitled "Tertiary and Palæozoic Traps," that he has nearly ready for publication the results of many investigations sustaining the view which he has "for some time maintained, as to the complete identity in composition and structure of eruptive rocks of widely separated geological periods."

[1] Hunt says the iron is protoxide, though determined under the form of sesquioxide in his analysis.

Teneriffe, the centre of the mountain may be trachytic when the sides around are basaltic. I found a similar fact in a dissected volcanic mountain of Western Oahu*. And at Mauna Loa, where the ejections are almost all basaltic, there are felspathic lavas at the very summit of the dome about the summit crater. A sight of the boiling movements of Kilauea in 1840 led me to explain this association in volcanic mountains on the principle of *liquation*—the felspathic material being the least fusible, and being therefore left at the centre, while the more fusible iron-bearing lavas were drawn off by the outbreaks through the different sides of the cone. The diversity, on this view, is not proof of diversity of origin.

Again, the two kinds of igneous rocks occur on a majestic scale over the Pacific slope of the Rocky Mountains. The trachytic rocks there appear, as stated by Richthofen, to be generally the older†. Clarence King, in his description of the Shoshone falls on Snake River, states that, out of 700 feet in thickness of igneous rocks exposed in the bluff, the lower 300 feet are made up of trachyte, while the upper 400 are of basalt, and that a continuous field of igneous rocks, mainly basaltic at surface, stretches over the country of Snake River for 300 miles or more. The outflows are of later date than the Miocene Tertiary. The fact of the very wide geographical distribution of the basalt on the Pacific slope appears to be good proof, as in the case of the dolerite of the Atlantic border (but better, because of the wider range of the ejections), that the source of its material was not local or dependent on the fusion of sedimentary strata. And if this is true of the basaltic rock, it is probably so also of the trachyte. Whatever doubt may exist, the general argument is made a demonstration by the fact explained‡, that a vast undercrust fire-sea was a necessity in order that the great heavings and bendings of the earth's crust essential to mountain-making on the Pacific border should take place.

The conclusion arrived at militates not only with the theory of Hunt, but also to some extent with Mallet's, unless the latter is made to appeal to the *true crust* for the material to be fused by the motion attending mountain-making. The motion in the true crust, even in the catastrophic period of mountain-making, is very much less and slower than that which is experienced by the plicating strata above it, and must therefore be a much

* See on this subject the author's 'Geol. Report, Wilkes's Exp. Exped.' pp. 204, 269, 368, 372.

† "The Natural System of Volcanic Rocks," Mem. Calif. Acad. Sci. vol. i. pt. 2 (4to, San Francisco, 1868). The trachytes of Auvergne are similarly older than the dolerites of the region. But more recent trachytes occur in Italy, and the rock of modern eruptions in Iceland is trachyte.

‡ *Suprà*, page 211.

feebler source of heat and fusion. Still, under the fractures and shovings and crushings which must at times take place, it should be sufficient; and acting at infra-Archæan depths, it would give uniform results over wide areas, or, on the other hand, a degree of diversity. Unlike Hunt's hypothesis, Mallet gives a reasonable source for the heat occasioning fusion.

But the sufficiency of the method for all cases of igneous ejections may well be questioned. The subsidence ending in the fissures and trap-ejections of the Atlantic slope from Nova Scotia to North Carolina was extremely slow, and probably nowhere exceeded 5000 feet; and it caused in the end only small displacements of the strata. In such a case as this the motion would seem to be a wholly inadequate cause of the fusion and ejections. Moreover, if the dependence of the subsidence upon the existence beneath of a great region of mobile rock was a fact, as has been urged*, there was fusion enough without aid from this source.

Further, over the Pacific slope of the Rocky Mountains the vast ejections in the Tertiary era appear to have had, as I have said, a natural source in an undercrust fire-sea, and the same *that was essential to all the previous oscillations of the crust,* and which therefore, like that beneath the eastern border of the continent, *must have been continued on from the period of general fluidity.*

Moreover these Pacific-border eruptions took place in connexion with, or as a consequence of, only a slight geanticlinal uplift (that is, slight compared with the extent of the region, although adding 10,000 feet to the height of the Rocky Mountains, since the angle of slope made by it was not over 15′), and through simple fractures that were unattended by flexings or crushings of the region broken—conditions wholly incapable, it would seem, of generating the heat required for so vast an amount of subterranean fusion as the ejections indicate.

Again, on both the Atlantic and Pacific borders of North America, wherever the plications have been greatest, and the conditions therefore favourable for producing the largest amount of heat, there we find evidences of the profoundest metamorphism and of the *least amount of fissure-eruptions*; and conversely, the regions of gentler plications and feebler metamorphism, or of none, are those of the most numerous fissures and most abundant igneous ejections. The Green Mountains are an example of the former, and the Triassico-Jurassic areas on the Atlantic border, or the Tertiary and Quaternary outflows on the Pacific slope, of the latter. The reverse should be true if the heat for the fusion were transformed motion; for fusion cer-

* *Suprà,* page 211 *et seq.*

tainly requires a much higher temperature than metamorphism. The evidence appears to be decisive against the making of the vast undercrust fire-sea by this method.

On the other hand, as already stated, there are in many countries regions of siliceous trachytes whose ejections may well have come from the local fusion of common granite and the allied schistose rocks, or of sedimentary strata of like composition.

Mallet's theory presents us with a true cause; but what are the limits of its action it is very difficult to decide. It relieves the theory of local fire-seas as derivative from the old viscous layer of the chief objection urged against it, that such isolated fire-regions could not long exist surrounded by cooled rock; for if inadequate to make great undercrust fire-seas one or more thousand miles in length, like the Appalachian, the cause may be sufficient through the generated heat to keep the old fire-seas in prolonged existence.

2. *What are igneous rocks?*—From the preceding discussion we derive an idea of the distinction between eruptive and metamorphic rocks. Since the larger part of eruptive rocks have come from the infra-Archæan region (either the true crust or the fire-seas within or below it), they are igneous in all their history, and in no sense metamorphosed sediments, whether derived from a second fusion of the rocks where they originated or not.

Again, the plastic rock-material that may be derived from the fusion or semifusion of the supercrust (that is, of rocks originally of sedimentary origin) gives rise to "igneous" rocks often not distinguishable from other igneous rocks when it is ejected through fissures far from its place of origin, while crystalline rocks are simply *metamorphic* if they remain in their original relations to the associated rocks, or nearly so.

Between these latter igneous rocks and the metamorphic there may be indefinite gradations, as claimed by Hunt. But if our reasonings are right, the greater part of igneous rocks can be proved to have had no such supercrust origin. The argument from the presence of moisture or of hydrous minerals in such rocks in favour of their origin from the fusion of sediments has been shown to be invalid.

3. *Source of the ejecting-force.*—When the fractures of the crust giving exit to fissure-eruptions are a direct sequence to a long continued subsidence (as, for example, in the case of the Triassico-Jurassic eruptions of the Atlantic border), there can hardly be a doubt that the lateral pressure causing the subsidence contributed also to the ejection of the plastic rock from beneath. And as the great fissurings of the crust are in all cases incidents in the working of lateral pressure, it is unsafe to deny that this

cause has aided in the great majority of eruptions in non-volcanic regions.

Another cause of ejection appealed to is pressure from the vapours imprisoned in the regions of fused rock. It must have often given efficient aid. But such vapours may not exist to the extent sometimes supposed about the deep sources of the material of fissure-eruptions. Non-volcanic igneous rocks are usually solid throughout, without the minutest vesicle; and similar complete compactness characterizes many fissure-ejections even of volcanic regions when they have taken place at a considerable distance from the volcanic vent*. This would hardly be so generally the fact if vapours were abundant about the sources of the ejected material; for such vapours would imply the existence in the mobile rock itself ordinarily of vaporizable ingredients capable of easy vaporization under the pressure there existing. The fact seems to be that the great pressure is in the way of vaporization of the sulphides that may exist in the plastic material; for pyrites is often found in the solid basalt. And, further, the deep-seated sources of igneous rocks must be mostly or wholly below the regions accessible to moisture; for if not, they would show its presence by hydration and frequently a vesicular structure.

Volcanoes in their states of *ordinary* activity and eruption do not appear to be dependent on the lateral pressure in the earth's crust. As I have long since urged, sustaining the view of Prévost, the force engaged is chiefly pressure from the expansion of vaporizable material rising with the lava. Besides this, there is the hydrostatic pressure of the liquid column raised in the conduit through the expanding vapours. The want of sympathy between the summit-crater of Mauna Loa, nearly 14,000 feet in elevation, and the larger crater of Kilauea on the flanks of the same broad mountain only 4000 feet above the sea, I have adduced as evidence that the ordinary volcanic action was here due to movements in the upper parts of the lava columns, probably to portions extending little below the sea-level, and that these volcanoes were therefore mainly dependent for their various phases on the freshwaters precipitated over the mountain-slopes. The waters of the ocean take their part in such action; but they are not in Hawaii the chief source of activity†.

* It is a great, though common, mistake to suppose that volcanic ejections are slags or scoria. The surface of an outflow is often of this character for a depth of 6 inches or perhaps a foot. But below this the layer is usually a compact stony mass with nothing slag-like, except that it is somewhat vesicular. About Hawaii, part of the rock is as solid and free from vesicles as the dolerite of the Connecticut valley.

† See further on these points the author's 'Expl. Exp. Geol. Report,'

The effects of hydrostatic pressure have been exemplified in the same volcanic mountain, not only in fractures of the mountain, but also in majestic fire-fountains, in which the lavas were thrown to heights of from 100 to 700 feet*.

Another Part, on the Formation of the Continental Plateaux and Oceanic Depression, will finish this memoir.

[To be continued.]

XXXIV. *On the Determination of the Specific Heat of Gases and Vapours at Constant Volumes.* By R. C. Nichols, *Esq.*†

THE specific heat of gases and vapours at constant pressure has been directly determined by experiment; but that at constant volume has hitherto been deduced from the former only by a somewhat complicated process of experiment and calculation. The relation between the specific heat at constant pressure c and that at constant volume c_1, for atmospheric air, thus obtained by Clément and Desormes is $\frac{c}{c_1}=1{\cdot}348$, by Masson 1·419, and has been calculated from experiments on the velocity of sound at Paris at 1·4122, giving for the specific heat of air at constant volume the values ·1763, ·1675, and ·1683, if the value for constant pressure be taken (as determined by Regnault) at ·2377.

The value of c_1, however, may be directly derived from that of c by a simple calculation. It is obvious that the equivalent in mechanical work of a given elevation of temperature at constant pressure must be equal to that at constant volume plus the work of expansion at constant pressure. Now this last is $p(\mathrm{V}_1-\mathrm{V})$, if p be the pressure per square foot, V and V_1 the volumes in cubic feet before and after expansion. And the expansion of air is ·003665 for every degree Centigrade above zero, or ·002036 for every degree Fahrenheit above 32°. For one degree above the temperature t, therefore,

$$\mathrm{V}_1-\mathrm{V}=\frac{{\cdot}002036\mathrm{V}}{1+{\cdot}002036\,(t-32)},$$

and

$$p(\mathrm{V}_1-\mathrm{V}_1)=\frac{{\cdot}002036p\mathrm{V}}{1+{\cdot}002036\,(t-32)}.$$

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and also various articles in Silliman's American Journal. Mallet's memoir, already often referred to, has excellent observations on this subject, as on others connected with volcanic action.

* Silliman's American Journal, S. 2. vol. xiv. pp. 219, 254 (1852).

† Communicated by the Author.

Now, if w be the weight of the air, the work of the elevation of temperature 1° at constant pressure is $772wc$, and at constant volume $772wc_1$; therefore

$$772wc = 772wc_1 + \frac{\cdot 002036 pV}{1 + \cdot 002036(t-32)};$$

and the weight of a cubic foot of air at 60° and a pressure of 14·706 lbs. per square inch being 535·68 grs.,

$$w = \frac{535\cdot 68 pV}{7000 \times 14\cdot 706 \times 144} \frac{1 + \cdot 002036(60-32)}{1 + \cdot 002036(t-32)};$$

therefore

$$c = c_1 + \frac{\cdot 002036 \times 7000 \times 14\cdot 706 \times 144}{535\cdot 68 \times 772 \times 1\cdot 057}$$

$$= c_1 + \cdot 06906$$

Or, if $c = \cdot 2377$, $c_1 = \cdot 1686$.

This difference, ·06906, between the specific heat of air at constant pressure and that at constant volume requires only to be divided by the specific gravity (air being 1) of any gas or vapour to obtain the corresponding difference for that gas or vapour. The specific heat of aqueous vapour at constant volume thus determined is $\cdot 837 - \cdot 1110$, or ·726.

XXXV. *On the Temperature and Physical Constitution of the Sun. (Second Memoir.)* *By* F. Zöllner*.

§ 1.

In a previous memoir on the same theme†, I endeavoured to ascertain boundary values for the *minimum* of the temperature at the surface of the sun and at a certain depth beneath it. The method of this determination of temperature had for its *theoretical* hypotheses the following:—

1. The law of Mariotte and Gay-Lussac;
2. The constancy of the ratio of the specific heats with constant volume and constant pressure;
3. The assumption that the eruptive protuberances are a phenomenon of the out-streaming of a gas.

* Translated from a separate impression, communicated by the Author, from the *Berichte der Kön. Sächs. Gesellschaft der Wissenschaften, math.-phys. Classe*, February 21, 1873.

† *Ber. d. K. Sächs. Gesellsch. d. Wiss.* June 2, 1870. Phil. Mag. S. 4. vol. xl. p. 313. Supplementary remarks will be found at the close of the present memoir and in *Natur der Cometen*, p. 490.

The *empiric* hypotheses were:—

1. The numerical value of that ratio of the specific heats;
2. The numerical value of the pressure of the hydrogen atmosphere at a definite height above the glowing liquid surface of the sun;
3. The numerical value of this definite height;
4. The density of the masses of hydrogen compressed in the interior of the sun, and breaking forth at its surface in the form of eruptive protuberances.

It is obvious that, the less the number of the hypotheses demanded by a method for the determination of physical properties of the sun on the basis of terrestrial units of measurement, the more probable will be the results it must furnish. I therefore take leave to communicate in the following a considerably more simple method for the determination of the temperature of the *atmosphere* of the sun—a method which for its employment requires, as a *theoretical* hypothesis, only the law of Mariotte and Gay-Lussac, and as an *empiric* presupposition only the knowledge of the *density-ratio* which subsists in two strata, at different altitudes, of the hydrogen atmosphere, the distance of which is known.

Let, namely,

h denote the distance of the two strata,
σ_1 the density in the lower stratum,
σ_2 the density in the upper stratum,
r the distance of the lower stratum from the centre of the sun,
g the intensity of gravity in the lower stratum,
α the coefficient of expansion of the gases at 1° C.,
a a constant, dependent on the nature of the atmospheric gas,
t the absolute temperature of the atmosphere under consideration;

then, as is well known, in the state of equilibrium, and presupposing a constant temperature, the following relation subsists between the above eight quantities:—

$$\log \text{nat.} \frac{\sigma_1}{\sigma_2} = \frac{gr}{a\alpha t} \cdot \frac{h}{r+h}. \quad \ldots \ldots \quad (1)$$

Putting herein

$$\log \text{nat.} \frac{\sigma_1}{\sigma_2} = l,$$

there follows as the expression for the absolute temperature:—

$$t = \frac{gr}{a\alpha l} \cdot \frac{h}{r+h}, \quad \ldots \ldots \ldots \quad (2)$$

or, if h can be neglected in comparison with r, as a formula of approximation,

$$t=\frac{gh}{a\alpha l}. \qquad (3)$$

§ 2.

The spectroscopic examination of the margin of the sun permits us to observe a portion of the incandescent hydrogen which forms an essential constituent of the sun's atmosphere, in the form of the so-called chromosphere, and to determine the mean altitude (h) of this stratum at those parts of the sun's margin which by the absence of protuberances lead us to infer a certain degree of equilibrium of the atmosphere. If, then, we were in a position to ascertain even *approximately* the ratio of the pressure or the density at the lower and upper boundaries of the chromosphere, we should thereby attain possession of both those empiric data which, by the employment of formula (2) or (3), would afford us the calculation of a mean value of the temperature for the chromosphere. Since the required ratio does not enter *directly* into the temperature-formulæ, but only its natural logarithm, the values of that ratio may vary within relatively wide limits without thereby considerably affecting the temperature-value dependent thereon. If, for example, that ratio varied between the values 500 and 5000, the natural logarithms of these values would be relatively 6·2 and 8·5; and therefore even so great an uncertainty in the ascertaining of the pressure-ratio would imply only a proportionally little alteration of the temperature-value which depends on it.

We have now to inquire if there are phenomena which permit us to infer an approximate value of the ratio of the pressure at the base and at the ordinarily visible limit of the chromosphere.

In my previous memoir on the temperature and physical constitution of the sun, the investigations of Frankland, Lockyer, Ste.-Claire Deville, and Wüllner on the changes in the spectra of gases with variations of the pressure formed the argument which, I thought, justified me in assuming "that the pressure at the base of the chromosphere, or at the extreme margin of the luminous disk of the sun, must lie between those of 50 and 500 millims. of mercury at the surface of the earth" (*l. c.* p. 110).

In a later memoir, however, "On the Influence of Density and Temperature on the Spectra of Incandescent Gases"*, I showed that, *cæteris paribus*, the increase of *density* of a luminous gas must produce precisely the same effect as the increase of the thickness of the luminous stratum—or, in other words, that

* *Berichte K. S. Ges. W.* Oct. 31, 1870. Phil. Mag. S. 4. vol. xli. p. 190.

the variations which the spectrum of a luminous gas at a constant temperature undergoes through variations of pressure do not depend on the value of the pressure or the density at a determined point in the mass, but only on the *quantity of the luminous particles* which send their light to the eye and therefore (no matter how distributed) are situated in the observer's line of vision.

This principle of *the spectroscopic equivalence of density and thickness of a radiant gas stratum* diminishes, as is readily perceived, the previously assumed values of the pressure at the base of the chromosphere, because at that time the modification of the hydrogen-spectrum, as observed under terrestrial conditions, was presupposed as a function of the pressure only, and not as a function of the density and thickness of the luminous stratum.

Hence, *if we wish to infer, from the accordance of the regular variations exhibited by the spectra of incandescent gases under terrestrial conditions with changes of pressure, the ratios of pressure or density in the atmospheres of incandescent heavenly bodies, we must always take into account the quantity of luminous particles which, lying in the observer's line of sight, simultaneously emit their light to his eye.*

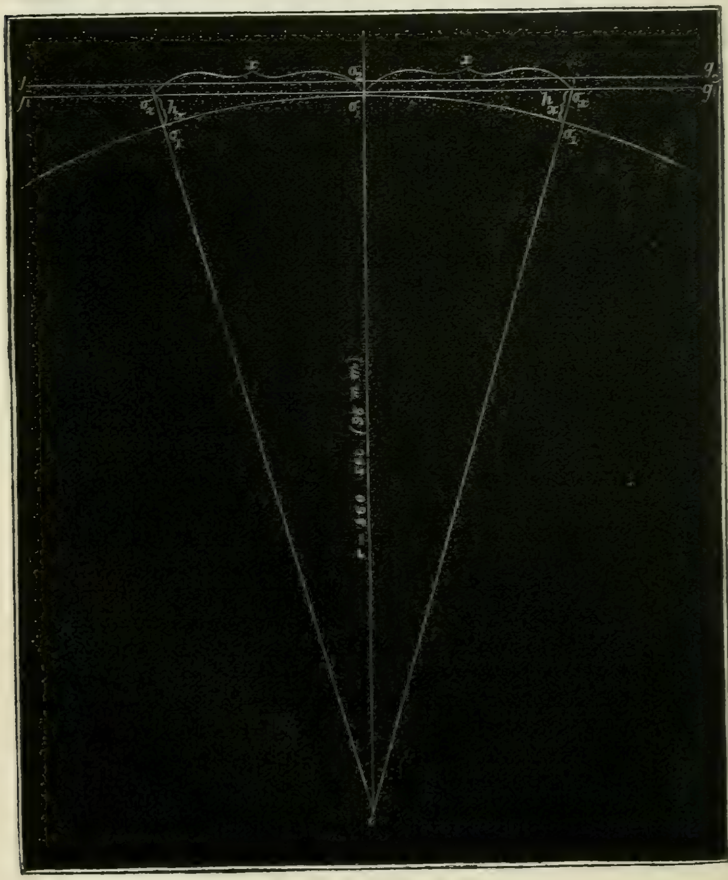
Thus, for example, let the spectrum of the hydrogen in the narrow channel of a Geissler's tube of 1 millim. internal diameter be compared with the hydrogen-spectrum of the chromosphere *simultaneously* in the same field of view, in order as far as possible to eliminate all heterogeneous influences of the illumination upon the judgment of the spectra.

If, now, we could by variations of the pressure produce in the line F, for example, all those variations of breadth which the same line has in the spectrum of the chromosphere at different distances from the base of the latter, we should be justified in assuming generally that like physical conditions are realized in the narrow channel of the Geissler tube as in the chromosphere —that is, that the *same temperature* prevails, and *the same number of incandescent particles* of gas lie *in the observer's line of view.* Hence, were the immense stratum of gas which is cut through by the visual line at the base of the chromosphere everywhere of equal thickness, then the values of the pressure or the density in the Geissler tube and at the base of the chromosphere would, under the condition mentioned, be in the inverse ratio of the thicknesses of the radiating layers. An idea of the ratio of the radiant gas-strata can also be obtained if we reflect that, under the above-made hypotheses, the whole layer of gas at the base of the chromosphere must be just as transparent as the thin column in the narrow channel of the Geissler tube. It is hence at the same time evident how slight must be the density already, in such close proximity to the body of the sun, of the gaseous matter surrounding it.

§ 3.

It can now be easily shown that in the present case, to a very close approximation, *the ratio of the quantities of gas particles which at the lower and upper limits of the chromosphere lie in our line of vision agrees with the ratio of the densities in the chromosphere at the same limits.*

Let $g_1 g_1$ and $g_2 g_2$ in the annexed figure denote the two visual lines which pass through the chromosphere at its lower and upper limits. The radius r of the sun's disk is drawn about 96 millims. long; so that, assuming a mean apparent semidiameter of the sun of 16′ or 960″, 1 millim. in the figure corresponds to an apparent magnitude of 10″ on the sun's disk. If, then, the mean altitude of the chromosphere be taken at from 10″ to 15″, it will be represented by the distance between the lines $g_1 g_1$ and $g_2 g_2$ in the figure.



If σ_x denotes the density of a volume-element of the chromosphere which on the visual line $g_1 g_1$ is at the distance x from the maximum of density σ_1, the quantity m_1 of gas contained in a space of the length $2x$ and the unit of surface as cross section is expressed by

$$m_1 = 2\int_0^x \sigma_x dx. \qquad (4)$$

With an unlimited atmosphere this integral, taken strictly, would have to be extended to the entire visual line—that is, as far as to the eye of the observer; but in the present case, considering partly what has been remarked, and partly the inexactness of the empiric data, we have a right to extend the integral only to such a length of the line x as makes it permissible, in view of the approximative character of the entire determination of value, to neglect the altitude h_x in comparison with r.

On this hypothesis we have

$$\sigma_x = \sigma_1 e^{-\frac{gh_x}{a\alpha t}}, \qquad (5)$$

or, putting for h_x the value $\frac{x^2}{2r}$, which results from the equation

$$(r+h_x)^2 = r^2 + x^2$$

by neglecting h_x^2,

$$\sigma_x = \sigma_1 e^{-\frac{gx^2}{2ra\alpha t}}. \qquad (6)$$

Putting herein

$$\frac{g}{2ra\alpha t} = c, \qquad (7)$$

and substituting in expression (4) the value then resulting for σ_x, it becomes

$$m_1 = 2\sigma_1 \int_0^x e^{-cx^2} dx. \qquad (8)$$

Without previously entering more closely into the determination of the value of this integral, it is readily seen that, with the smallness of the distance between the two lines $g_1 g_1$ and $g_2 g_2$, the following expression results for the number of gas particles situated on the upper line $g_2 g_2$:—

$$m_2 = 2\sigma_2 \int_0^x e^{-cx^2} dx. \qquad (9)$$

By division we get

$$\frac{m_1}{m_2} = \frac{\sigma_1}{\sigma_2}, \qquad (10)$$

or, expressed in words:—

The quantities of luminous gas particles which at the lower and upper limits of the chromosphere lie on the line of sight of the observer, have approximately the same ratio as the densities or pressure-values in the strata in question of the chromosphere. Q. E. D.

§ 4.

The application of the hitherto developed formulæ to the temperature ratios on the surface of the sun presupposed in reality only the knowledge of the ratio of the pressure or density in two strata at different altitudes of the chromosphere, the distance between which is known; the pressure or the density itself at those altitudes remained undetermined. It shall now be shown how, with the aid of the above formulæ, this quantity also can be approximately calculated.

Formula (8) expressed the quantity m_1 of the incandescent gas contained in a space whose length is formed by the extent passed through at the lower limit of the chromosphere by the line of sight, and whose cross section is the unit of surface.

Putting herein

$$cx^2 = y^2,$$

the expression for m_1 changes into the following:—

$$m_1 = \frac{2\sigma_1}{\sqrt{c}} \int_0^{\frac{y}{\sqrt{c}}} e^{-y^2} dy. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

The integral which here enters cannot be presented in a definite form; on the contrary, it is known from a formula which expresses the probability that a designated error of observation does not exceed a certain amount. For constantly increasing values of y, the integral converges towards the value $\frac{\sqrt{\pi}}{2}$, so that we have

$$\int_0^{\infty} e^{-y^2} dy = \frac{\sqrt{\pi}}{2}.$$

From the Tables which contain the numerical values of the above integral for increasing values of y it is evident that the convergence is rapid, so that *e. g.* for $y = 2.4$ the value of the integral differs by less than a thousandth part of its quantity from $\frac{\sqrt{\pi}}{2}$. We shall therefore in the present case be justified in substituting this value as sufficiently approximate for the above integral and obtaining

$$m_1=\sigma_1\sqrt{\frac{\pi}{c}}$$

or

$$\sigma_1=m_1\sqrt{\frac{c}{\pi}}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Substituting for c its value from equation (7) and putting

$$\frac{1}{\alpha}=273,$$

we get

$$\sigma_1=m_1\sqrt{\frac{273g}{2r\pi at}}. \quad . \quad . \quad . \quad . \quad . \quad (13)$$

This formula shows that the density (*i. e.* the mass contained in a unit of volume) in a definite stratum of the chromosphere can be calculated approximately, if the mass m_1 contained in the above-defined space and the absolute temperature of the same are known.

§ 5.

The expressions above found are worthy of notice for the purpose of applying spectroscopic observations to the temperature-relations of the sun; for, provided we could with constant temperature, observing the above-mentioned precautions, by varying the pressure of electrically luminous gases produce those modifications of the hydrogen-spectrum, for example, by which the two limits of the chromosphere are optically determined, we should be justified in presupposing in the sun's atmosphere also the ratio of pressures herein found, and in this way, with the aid of formula (1) or (3), ascertaining a numerical value for the temperature of the stratum in question of that atmosphere.

As is well known, the difficulties of fulfilling experimentally the conditions here required are so serious on this account—because in general the electrical resistance becomes greater as the density of the gas increases, so that the greater quantity of electricity necessary to overcome it produces at the same time a higher temperature of the glowing gas.

Wüllner has observed the spectrum of hydrogen under a great variety of pressures. In his first memoir on this subject he remarks:—

"With 6 millims. pressure, besides the characteristic hydrogen-lines, the orange part was still just visible, ...

"On the gas being still further rarified, to 3 and then to 2 millims., the characteristic lines retained the same brightness, all the rest vanished almost completely from the spectrum. Simultaneously, however, with a weakening of the bright lines

there reappeared a portion of the continuous spectrum in the green, in the form of perhaps five bright fields, when the gas was rarified to *fractions of a millimetre* pressure"*.

In a subsequent memoir, "On the Spectra of some Gases under high Pressures"†, Wüllner observed the spectrum of hydrogen up to a pressure of 2240 millims., the highest which his apparatus permitted. The most essential characters of the spectrum with 1703 millims. pressure are described as follows:—

"With this pressure H_α has already lost much of its sharpness; it shows itself as a band of several minutes breadth, at the edges of which the intensity of the light diminishes quickly. . . .

"The green portion of the spectrum shines very bright; the brightness increases at first more slowly, then more quickly as far as the place of H_β, where the spectrum is brightest, so that this part appears almost white. In the direction of the blue the brightness lessens quickly; yet the blue and violet are very beautiful, most so in the region of H_γ, so that, compared with the appearance with a lower pressure, H_γ seems to reappear, though without sharpness and widened like H_β. On this account the boundary of the spectrum goes a little beyond the place of H_γ."

Passing then to the highest pressure, he remarks as follows:—

"By a still further augmentation of the pressure a much nearer approach to a continuous spectrum could not be attained; even with a pressure of 2240 millims., or almost 3 atmospheres (the highest permitted by the dimensions of the apparatus), H_α still persisted in a similar fashion; the loss of sharpness at the edges, however, had proceeded so far that we may expect with a still further increase of pressure to see H_α disappear, just as H_β and H_γ have already vanished under less pressures" (*l. c.* p. 342).

In his most recent paper "On the Spectra of Gases in Geissler's Tubes" (Pogg. *Ann.* vol. cxlvii. p. 347), M. Wüllner seems inclined to assume that the change described by him in the above extracts, of a discontinuous into a continuous spectrum is "essentially dependent on the rise of the temperature." Among the reasons which compel him to such an assumption, he alleges the following:—

"That it is in reality the rise of the temperature which determines the formation of the continuous spectrum is also spoken for by the fact that in most cases it is accomplished not by a widening of the bright lines, but by the illumination of the entire background constantly growing brighter, without the lines mean-

* Pogg. *Ann.* vol. cxxxv. pp. 305 & 306.

† Pogg. *Ann.* vol. cxxxvii. p. 337. Phil. Mag. S. 4. vol. xxxix. p. 365.

while losing much of their sharpness; for an increase of the density without a rising temperature can only, it seems to me, effect the change into a continuous spectrum by widening the lines, while by a rise of temperature, and therewith an increase of the emissive power for all wave-lengths, the spectrum can just as well become continuous in the other way."

As is seen, these remarks do not touch, at all events, the observations of Wüllner's I have quoted above, since the widening of H_α to a "band of several minutes breadth which has already lost much of its sharpness," and "at the edges of which the intensity of the light diminishes quickly," is expressly particularized as a prominent feature. I believe, therefore, that at least *these* observations can only be interpreted as they have been in my theoretical investigation "On the Influence of Temperature and Density on the Spectra of Incandescent Gases," viz. "that the spectra of gases in Geissler's tubes becoming continuous may be a consequence of an increase of the density of the gas, as well as of its temperature being raised."

The share of each of these two causes in the observed widening of the lines and the continuity of the spectrum must of course remain undecided until the dependence of the emissive and absorptive power on the temperature of the gas in question can be ascertained.

§ 6.

If I now pass to a nearer discussion of the conditions under which the knowledge of the necessary numerical quantities can be attained, and at the same time make certain substitutions on the ground of the hitherto roughly approximate values, I here remark explicitly that I do so more for the purpose of illustrating the theory developed than of deriving definitive values. This remark is the more necessary as the temperature-values obtained in a previous memoir, which I expressly designated as *minima*, have been erroneously taken as definitive determinations.

The observations above given in detail, on the modifications of the hydrogen-spectrum with variation of the density and temperature, will in some measure put the reader in a position to form an independent judgment on the question how far we have a right to compare these variations to those which we observe in the spectrum of the chromosphere. Considering that the diversities of temperature at the lower and upper boundaries of the chromosphere have a continual tendency to disappear through the manifold and violent movements therein, while the movements themselves owe their origin partly to those differences of temperature, we may venture to ascribe the widening of the line H_β, for example, at the base of the chromosphere in reality to

an augmentation of the density and thickness of the radiant stratum of gas. Hence, in consideration of the proposition above demonstrated (see p. 296), we can assume as the ratio between the densities at the lower and upper boundaries of the chromosphere the approximate *ratio* between the values of those pressures within which, under terrestrial circumstances, the spectrum of hydrogen undergoes analogous changes in its constitution. According to Wüllner's above-cited experiments, those values would be, in round numbers, 2240 millims. and 1 millim. Now, as the mean altitude of the chromosphere at the most tranquil places on the surface of the sun may, according to the observations, be taken at about 10 seconds of arc, we should be in possession of those two numerical data which, with the help of formula (3), furnish an approximate mean value for the absolute temperature of the chromosphere.

The formula was

$$t = \frac{gh}{a\alpha l},$$

in which

$$l = \log \text{nat.} \frac{\sigma_1}{\sigma_2}.$$

Taking as units the metre and the centesimal degree, we have then

$$g = 274{\cdot}3, \qquad h = 7153300,$$

$$\frac{1}{\alpha} = 273, \qquad \frac{\sigma_1}{\sigma_2} = 2240.$$

$$a = 1131600 \text{ (hydrogen)},$$

Herein

$$a = \frac{\rho_1 g_1 a_1}{\rho},$$

if

ρ_1 is the specific gravity of mercury,
g_1 the intensity of gravity at the surface of the earth,
a_1 the mean barometric pressure,
ρ the density of hydrogen under this pressure at 0° C.

The resulting value of the absolute temperature is

$$t = 61350°.$$

Here let me again give prominence to the circumstance that the uncertainty of the numerical determination of the ratio $\frac{\sigma_1}{\sigma_2}$ affects the value of t only to a very small degree, because *e. g.* a

value 10 times as great, the rest of the assumptions remaining unaltered, would only lower the calculated temperature to

$$t=47270^\circ.$$

Both from this circumstance, and from the simplicity of the theoretical hypotheses, the formula here given may claim an essential superiority over the way I formerly proposed for the determination of the temperature of the sun. Besides, it exhibits a remarkable relation between the quantities t and h. If, namely, the altitude of the chromosphere were really determined *only* by the ratio of the densities at the lower and the upper boundary, the distance h of the two strata in which this constant pressure- or density-ratio exists would change proportionally to the absolute temperature. Hence, if we had numerous observations, to be treated statistically, on the mean altitude of the chromosphere at all parts of the solar margin, we should hereby, without knowing the temperatures themselves, be able to approximately judge of their ratio at different parts of the sun's surface—for instance, at the equator and the poles.

The small altitude of the chromosphere at the poles of the sun found by Secchi to correspond with his observations seems, in conjunction with the other results of the same observer, on the slight heat-radiation of the polar regions of the sun, to confirm the above-deduced relations*. For instance, at parts of the sun's surface where the mean height of the chromosphere amounts to about 15″, on the above-made hypotheses an absolute temperature of about 90,000° would result. But, however important for investigations of the temperature-proportions on the solar surface the proportionality between the temperature and the altitude of the chromosphere may become in the future, for obvious reasons the significance of this connexion need not be overestimated beforehand.

In order now to illustrate the applicability of formula (11) by the insertion of numerical values, let it in the first place be remembered that the comparability of the phenomena observed in Geissler's tubes with those in the chromosphere is only admissible on the hypothesis that the essential conditions on which the phenomena compared depend agree in the two cases within certain limits. As such conditions two have been substantially recognized—namely, first, the temperature, and, secondly, the quantity of the gas-mass which is encountered by the observer's visual line in equal cross sections of the spaces passed through.

* The depression of the chromosphere over the sun-spots, observed by Respighi might likewise be ascribed to a lowering of the temperature at those places.

It must consequently be an admissible hypothesis that the temperature and the quantity of hydrogen particles lying on the visual line in a Geissler's tube agree with the analogous quantities at those places in the chromosphere the spectrum of which corresponds with that of the Geissler's tube.

It was shown above, in relating Wüllner's experiments, that in general an increase of temperature operates in the same direction, in relation to the widening of lines in the hydrogen-spectrum, as an increase of density. So long, therefore, as the magnitude of the effect of each of these two causes by itself is not more exactly known, it would be conceivable that the same appearance of the spectrum may take place with different values of the temperature, if the corresponding values of the density and thickness are present. But if one reflects on the considerable brightness of the lines which in the chromosphere, in the immediate proximity of the brilliant margin of the sun, yet stand out distinctly from the strongly illuminated ground of the spectrum, he will be compelled to admit that the temperature of the glowing hydrogen in the chromosphere is at all events not below that at which the hydrogen gas in a Geissler's tube exhibits the same sharpness of the lines of the discontinuous spectrum. He will hence be obliged also to suppose that the mass of the gas passed through in the chromosphere by the visual line of the observer is rather less than greater in comparison with that looked through in the Geissler's tube.

It is now easy, with the aid of the previously developed formula (12),

$$\sigma_1 = m_1 \sqrt{\frac{c}{\pi}},$$

to derive a value for the density of the glowing hydrogen in a defined stratum of the chromosphere. If we select the lower boundary of it, in the above formula

σ_1 denotes the density (that is, the mass of hydrogen contained in the unit of volume) in the lowest stratum of the chromosphere,

m_1 the mass of hydrogen which, at the base of the chromosphere, is passed through by our visual line, if the cross section of the extent looked through is equal to the unit of surface.

After the foregoing discussions this mass must, at equal temperature, be supposed to correspond with that which, with equal cross section, lies on the spark-track perpendicularly looked through in a Geissler's tube.

If the space occupied by the gas particles which are encoun-

tered and rendered incandescent by the spark is very small in proportion to the whole space within which the passage of the spark takes place (as it was in Wüllner's more recent experiments, in which the tubes employed possessed throughout a clear width of about 2 centims., and the electrodes were distant 7·5 centims. from each other*), the extent of gas made incandescent will, under an approximately constant pressure, expand, and, in correspondence with Gay-Lussac's law, diminish its density. From this it is evident that for the estimation of the quantity of gas which lies on the visual line passed through perpendicularly by the spark-track, the knowledge of three quantities is required, viz.:—

1. The original density of the gas within the tube;
2. The thickness of the spark-track;
3. The temperature of the spark.

The first of these can be determined, the second approximately estimated for the present purpose; the third remains unknown. Yet, considering that the condition for the comparability of the terrestrial hydrogen-spectrum and that of the chromosphere rests on the assumption that the physical proportions are substantially the same in both phenomena, the temperature deduced above for the chromosphere must, by way of approximation, be presupposed also for the spark-track in the Geissler's tube. Lastly, the densities of the gas during and before incandescence would then be inversely as the absolute temperatures in the two states. Taking for granted, then, the temperature-values obtained above for the chromosphere, if the temperature of the hydrogen in the Geissler's tube before incandescence equals that of melting ice, the density of the glowing spark-track would be about $\frac{1}{200}$ of that of the gas contained in the tube at 0° C.

If it be further assumed that the thickness of the spark-track amounts to about 1 millim., the metre having been employed as the unit of length in the above formulæ, the density of a quantity of incandescent hydrogen which in the unit of volume contains the same mass as an extent passing rectilinearly right through the chromosphere with the unit of surface as cross section, will be only $\frac{1}{200000}$ of the density of the hydrogen at 0° C. in the Geissler's tube. Hence, if σ_0 denotes the original density, it is reduced in the spark-track of the assumed thickness to $\frac{\sigma_0}{200}$, and must be further diminished to the thousandth part of this quantity if expanded from the assumed extent of millimetre to that of a metre as the unit of length. The value

* Pogg. *Ann.* vol. cxlvii. p. 325.

so obtained, $\frac{\sigma_0}{200000}$, would then have to be put equal to m_1 in the above formula—that is, equal to the mass of gas contained in the unit of volume which quantitatively and qualitatively corresponds with the glowing hydrogen situated in a cylinder of the chromosphere parallel to the visual line and with the unit of surface as cross section.

We then obtain for the density at any place in the chromosphere, *e. g.* at its base, the following expression,

$$\sigma_1 = \frac{\sigma_0}{200000}\sqrt{\frac{c}{\pi}},\text{—}$$

or for the pressure, putting for σ_1 and σ_0 the pressure-values p_1 and p_0 proportional to them,

$$p_1 = \frac{p_0}{200000}\sqrt{\frac{c}{\pi}};$$

hence, since, according to the preceding,

$$c = \frac{273g}{2rat},$$

$$p_1 = \frac{p_0}{200000}\sqrt{\frac{273g}{2\pi rat}}.$$

If we take for p_0 the value of the highest pressure employed by Wüllner, and for t the first of the values found above, we get

$$p_1 = 0{\cdot}00000000016 \text{ millim. mercury.}$$

Calculated from this for the assumed temperature of incandescent hydrogen, the density at the base of the chromosphere is found to be about $(\frac{1}{10})^{19}$ of that of water. A hollow sphere of the size of our earth, filled with gas of this density, would represent a mass of about 84 cubic metres of water. If, then, the chromosphere with an altitude of $10''$ had everywhere this constant maximum density of its base, its total mass would only amount to $(\frac{1}{10})^{15}$ of the mass of the earth. Even with a daily renewal of the entire chromosphere, according to this calculation it would take three million years to consume a mass of hydrogen corresponding to about a millionth part of the mass of the earth, and which may therefore, relatively to the mass of the sun and its atmosphere, be regarded as, to our perceptions, perfectly infinitesimal.

[To be continued.]

XXXVI. *On the Moon's Libration.*
By W. R. Birt, *F.R.A.S., F.M.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

May I trespass on your kind indulgence for a portion of your columns to rectify a mistake into which, as it appears to me, Mr. Proctor has fallen in preparing his work on the Moon, recently published, or at least to supply an omission? In his preface he says, "In Chapter III. I give amongst other matters a full explanation of the *effects* due to the lunar librations. I have been surprised to find how imperfectly this interesting and important subject has been dealt with hitherto. In fact I have sought in vain for any discussion of the subject with which to compare my own." If Mr. Proctor confines his attention to the effects (I have italicized the word in my quotation) of libration only, the portion of the work which treats of them is not entirely original as he would indicate, inasmuch as the operation of libration in producing the elliptical motion of the point of intersection of the moon's first meridian and equator round the centre of the apparent disk was shown by me in Appendix II. of the Report of the Lunar Committee of the British Association for the Advancement of Science in 1866; but if Mr. Proctor alludes to the subject of libration generally, then it would appear that he has entirely overlooked the investigation of Encke in the *Berliner Astronomisches Jahrbuch für* 1843, pp. 283–293. It is the method of Encke that enters as an element into the computation of points of the first order; and if I remember rightly, Mr. Proctor has not made the slightest allusion either to these points or to the method of computing libration, these omissions constituting defects in his work which we should hardly expect to find in a writer of his ability. The following quotation from the Appendix before mentioned will show that Mr. Proctor has merely worked out the principle which I set forth in 1866 (Report Brit. Assoc. 1866, pp. 231–233); it should be read in connexion with that portion of his work contained in pp. 173–199:—

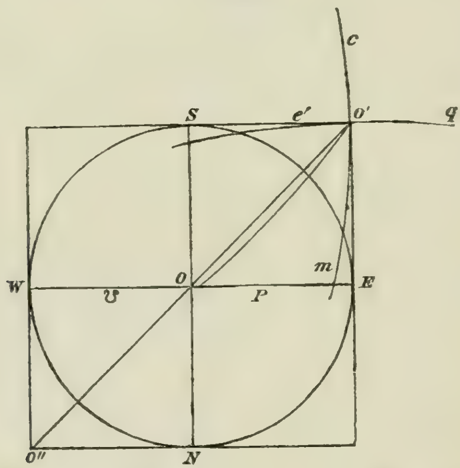
"*Application of the foregoing investigations to the motion on the apparent disk of the point at which the Equator intersects the First Meridian.*

"It now remains to inquire how the point of intersection of the moon's equator and first meridian will be affected by the changes in latitude and longitude which the centre of the apparent disk is perpetually undergoing; for as only the latitude and longitude of this single point are determined by the formulæ for

computing the librations, we do not appear to have at present the means for tracing out on the moon's disk the curves representing the moon's equator and first meridian for any other epochs than that of mean libration, when, as before mentioned, they cross the disk in two straight lines intersecting at the centre; and this inquiry is perhaps the more important as showing how necessary it is, for accurately mapping the surface, to have good determinations of points of the first order. Taking, therefore, the spot on the moon's surface at which the equator and first meridian intersect each other, we may inquire the path it will describe on the apparent disk during the changes of libration through one revolution of the nodes.

"In fig. 9 let W E N S represent a small circle concentric with the limb or margin of the apparent disk of the moon, W E being

Fig. 9.

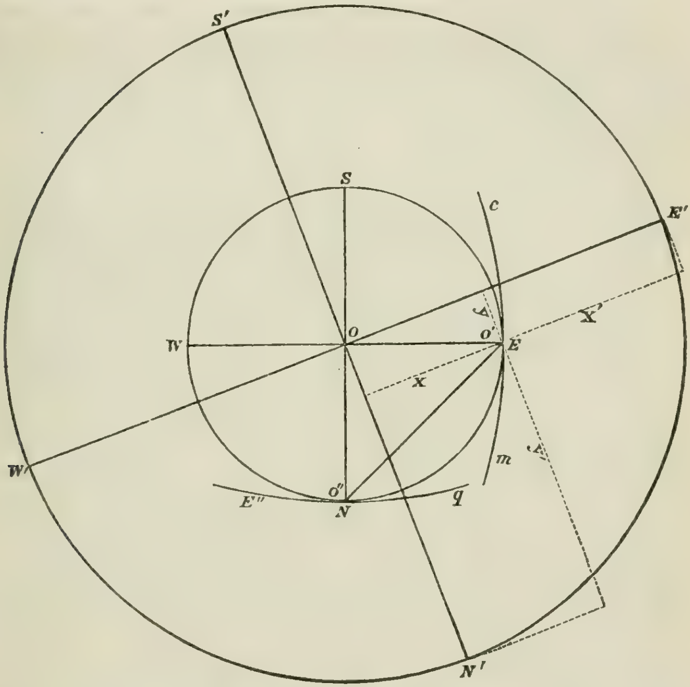


a portion of the equator, and N S of the first meridian in mean libration at the passage of the descending node and perigee respectively, and o the point of intersection of the two (0° of latitude and longitude), and o' the position occupied by the point o by the joint effect of both librations, o E will consequently represent the greatest excursion of the point o in longitude, and o S that in latitude, the equator being projected in the curve $e' o' q$, and the first meridian in $c\, o' m$. The displacement of o being in the line $o\, o'$, the libration of the centre of the apparent disk σ will be W in longitude and N in latitude. It is easy to see that the path of the point of intersection of the equator and first meridian, a short time before and after the epoch of mean libration, will be in a very narrow ellipse, the line $o' o''$ being the major

axis, which does not, however, retain its position on the apparent disk, but revolves around the central point.

"This ellipse opens out and undergoes changes of form proportional to the interval elapsing from the epoch of mean libration until the epoch when the greatest excursion of libration in longitude towards the east (of the point of intersection of the equator and the first meridian) coincides with the passage of the ascending node when the equator is represented as a straight line across the apparent disk and the first meridian by the curve c E m in fig. 10, where the libration of the centre of the apparent

Fig. 10.



disk is nothing in latitude, but west in longitude. When the first meridian returns to its normal position, the equator is represented by the curve E'' N q (fig. 10), and the point of intersection is situated at o'' (nearly); the libration of the centre in this case is nothing in longitude but south in latitude.

"At this epoch, intermediate between two of mean libration, the path of the point of intersection of the equator and first meridian may be represented by the four diagonals, of which o' o'' (fig. 10) is one, or, perhaps more correctly, by a wavy ellipse;

for as the values of the two librations differ in amount, the circle W E N S is not a true representation of the excursions of the intersecting point E and W, N and S; so when the greatest deviation from mean libration occurs, the real path of the intersecting point on the apparent disk is a wide ellipse, which gradually contracts to a narrow ellipse as the epoch of mean libration is approached. This will be the case proportionally with every point on the apparent disk, and the displacement will be in every possible direction and at every conceivable angle with the centre of the apparent disk. This suggests that by far the most effective mode of determining positions on the moon's surface is by measures for points of the first order; for let x' y' represent the measures in right ascension and declination from the east and north limbs of the point E, x and y will be the corresponding rectangular coordinates necessary to determine the selenographical position when the librations of the centre and the other elements are ascertained."

There are some other defects which I have noticed. The inclination of the moon's equator to the ecliptic is given as $1° 30' 11''$, the text-book's value. Mr. Proctor, who finds great fault, and properly so, with text-book repetitions, seems to be unaware that Wichmann's determination, $1° 32' 9''$ (*Ast. Nach.* No. 631), is used in the Nautical-Almanac Office.

Mr. Proctor mentions the lunar seasons; but he omits, if I mistake not, the point at which each commences. Thus in the northern hemisphere spring commences when $\odot - \Omega = 0°$, summer when $\odot - \Omega = 90°$, autumn when $\odot - \Omega = 180°$, and winter when $\odot - \Omega = 270°$.

W. R. Birt.

XXXVII. *Specific-gravity Bottle for Liquids spontaneously Inflammable in contact with Air.* By Alfred Tribe, *Esq.**

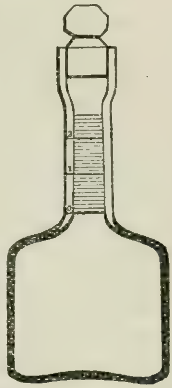
The bottle usually employed for specific-gravity determinations of liquids consists, essentially, of a light flask provided with a perforated stopper. By means of this arrangement sufficiently accurate results can be readily obtained when the liquid is not very volatile or violently acted upon by the air.

To meet the requirements of volatile liquids, Regnault employed a flask having a solid in place of a perforated stopper, and a neck somewhat longer and narrower than the old form. The liquid is poured up to a mark on the neck; and the water-value of this being known, the specific gravity is found. It is obvious that by this method loss by evaporation or expansion during weighing is prevented.

* Communicated by the Author, having been read at the Meeting of the British Association held at Bradford, September 1873.

For liquids, however, which are violently decomposed by the air, and especially those in which thick clouds are also formed (*e. g.* the zinc compounds of the lower members of the alcohol radicals), this plan, from the practical impossibility of adjusting with accuracy to the given mark and the required temperature, is not so satisfactory as might be desired.

To suit liquids of this class I have devised the following improvement upon Regnault's bottle:—The neck is of as even bore as possible, and divided into as many equal parts as can be conveniently read. The bottle* actually employed, which answered perfectly, has a capacity of 2·6 cubic centims., the neck being about 3 millims. internal diameter and 13 long, divided into half millimetres. Just beyond the graduations the neck is widened somewhat for the stopper and for pouring in the liquid.



When once the water-values have been determined for each division on the neck, it will be seen that it is only necessary to fill the bottle so that the surface of the liquid shall fall within the range of the graduations. Another advantage is that the contents can be raised or lowered to the normal temperature, and the volume read off without addition or subtraction of liquid.

The half-millimetre divisions of water weighed 5 milligrms. As it is easy to read to a half of one of these divisions, and with care to a quarter, the error need not be more than the weight of liquid equal to one or two milligrammes of water.

A pipette with a capillary tube will be found convenient for introducing the liquid; and of course the operation of filling with liquids of the character of zinc-ethyl should be done in the absence, as far as possible, of free oxygen.

XXXVIII. *On the received principles of Hydrodynamics, in reply to* Mr. Moon. *By* Professor Challis, *M.A., LL.D., F.R.S., F.R.A.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

On reconsidering my argument in the August Number relative to Mr. Moon's objections to the received principles of Hydrodynamics, I found that it might be more clearly exhibited by being generalized in the following manner.

* Made by Cetti.

Let p be the pressure, ρ the density, and u, v, w the resolved parts of the velocity in the directions of rectangular axes in any perfect fluid at any point whose coordinates are x, y, z at the time t. Then regarding it as an axiom that the state of the fluid at all points and at all times admits of being analytically expressed, the complete solution of every hydrodynamical problem will conduct to expressions for the five quantities p, ρ, u, v, w, inasmuch as, when these have been obtained as functions of coordinates and the time, the pressure, density, velocity, and direction of the velocity are determined for each point at each instant Consequently the solution will consist of five equations, such as

$$p=f_1(x,y,z,t),\quad \rho=f_2(x,y,z,t),\quad u=f_3(x,y,z,t),$$
$$v=f_4(x,y,z,t),\quad w=f_5(x,y,z,t).$$

The elimination of x, y, z, and t from these equations would conduct to an equation of condition, $F(p, \rho, u, v, w)=0$, between the five quantities. This result proves that in the solution of every hydrodynamical problem there exists between the quantities determined a relation the analytical expression of which does not contain explicitly the coordinates and the time.

Now the condition thus shown by *à priori* considerations to be necessary can be satisfied by an *arbitrary* relation between the quantities—that is, by one which is independent of the particular problem. For by means of such a relation it will be analytically possible to eliminate from the general hydrodynamical equations one of the unknown quantities, as, for instance, p. Then in any particular instance the integration of the equations gives the values of the other four as functions of the coordinates and the time. These being found, the value of the first is obtainable from the assumed relation. By this process the state of the fluid, subject to the arbitrary condition, will be ascertained, in the proposed instance, for all points at all times. The arbitrary condition has the effect of *defining* the fluid; and evidently the number of different kinds of fluid is unlimited.

It having been thus proved to be allowable to assume in hydrodynamics an arbitrary relation between p, ρ, u, v, w, let us suppose, for illustration, that

$$p=A\rho^{\alpha}+Bu^{\beta}+Cv^{\gamma}+Dw^{\delta},$$

the coefficients A, B, C, D and the indices α, β, γ, δ being constant and arbitrary. By this equation p might be eliminated from the general hydrodynamical equations, so that the integrations of the equations for a particular problem would give expressions for ρ, u, v, w as functions of the coordinates and the time, whence that for p might be obtained by means of the above equality. As the coefficients and the indices are wholly arbitrary

the reasoning would hold good if we supposed that $A=a^2$, $B=0$, $C=0$, $D=0$, and $\alpha=1$, or that $p=a^2\rho$.

This argument proves that it is allowable to make for fluid in motion the hypothesis that the pressure varies as the density always and at all points. The selection of this hypothesis is suggested by the circumstance that, as is known from experiment, it expresses the relation between the pressure and the density for fluid of given temperature at rest. The complete verification of the hypothesis would depend upon making a sufficient number of satisfactory comparisons of consequences mathematically deduced from it with experiment.

The foregoing argument receives confirmation by applying a similar one to an incompressible fluid. In this case, the density being constant, there are only the equations which give the values of p, u, v, w, the number of which does not exceed the number of the variables x, y, z, t. Consequently, as is otherwise known, there is not necessarily a relation between p, u, v, w which is independent of coordinates and the time.

In my communication in the August Number I gave Mr. Moon credit for the originality of the process of reasoning by which he obtained the equation $p=f(\rho, v)$, which is the same as the above equation $F(p, \rho, u, v, w)=0$ restricted to motion in one dimension; and I pointed out that this equation was inclusive of every relation between p, ρ, and v by which the fluid might be defined. Also I argued that Mr. Moon's reasoning is necessary for proving the legitimacy of assuming that $p=a^2\rho$ for fluid in motion. It will be seen from the additional arguments adduced in this letter that I still maintain these views, notwithstanding that I gather from Mr. Moon's communication in the September Number that he dissents from them. The reasons he there gives for his dissent do not appear to me to invalidate in any respect my arguments. To show this it may suffice to advert to two points on which he seems mainly to rely for his objections to the acknowledged principles of hydrodynamics.

(1) Mr. Moon founds an argument on the possibility of the immediate juxtaposition of two densities one of which is double the other; in other terms, he admits that $\frac{dp}{\rho dx}$ may have an infinite value. But the *à priori* demonstration of the law of the equality of pressure of a perfect fluid in all directions from a given point, *on which law the whole of analytical hydrodynamics depends*, excludes infinite values both of the effective accelerative force and of $\frac{dp}{\rho dx}$. See 'Principles of Mathematics' &c., pp. 106 & 173. There are, it is true, physical conditions under which

that differential coefficient must be extremely large; as in the rapid expansion of the tails of comets, and in explosions from gaseous mixtures; but it does not appear that there can be *in rerum naturâ* circumstances under which its value is absolutely infinite.

(2) The dilemma respecting the immobility of the stratum immediately beneath the piston is fairly inferred, and demands an explanation. I believe that in the work already cited I have furnished the means of explaining it. One of the definitions of a perfect fluid given in pp. 104 & 171 is, that its parts press against each other and against any solid with which they are in contact. Accordingly there is mutual pressure between a solid and fluid in contact such as there is between contiguous parts of the fluid, and *pro tanto* the solid may be considered a continuation of the fluid. Now when any heavy mass is at rest, if we conceive a horizontal plane to be drawn through it, the total upward pressure in this plane must be equal to the weight of the part of the mass above it, because this part is thereby just sustained. Hence from the top to the bottom of the mass there will be a gradual increment of pressure. If now another mass be placed on the first, there will be a like increment of pressure from the top of the upper to the bottom of the lower mass, and the pressure and gradation of the pressure in the latter will not be the same as before. Applying these considerations to the case of the piston, it will be seen that before putting on the additional weight there will be equality of pressure on the opposite sides of the plane of contact of the fluid and the piston; but as soon as the weight is added, there will be a tendency to an excess of pressure on the upper side due to the disturbance of the pressure in the solid piston; but as no finite difference of pressure can subsist, this tendency will give rise to a common acceleration of the parts of the fluid and solid that are immediately contiguous to each other. Thus an acceleration would commence at the upper surface of the stratum, and the equality of the pressures at the upper and under surfaces would cease to exist. The amount of the initial acceleration is determined by that of the descending solid.

Cambridge, September 22, 1873. J. CHALLIS.

XXXIX. *Notices respecting New Books.*

The Moon, her Motions. Aspect, Scenery, and Physical Condition. By RICHARD A. PROCTOR, *B.A.* London: Longmans and Co.

WE are somewhat disappointed in this work. Conceived on an excellent plan, with abundant materials at hand for producing an exhaustive treatise on the moon, the author has failed to

give in the compass to which he has restricted himself a condensed epitome of all the information within his reach alike useful for the student and general reader. The chapters to which we especially allude are those referring to libration, and the study and condition of the moon's surface. From the remarkable sentence in the preface, to the effect that nothing exists comparable to the author's exposition of libration, we fully expected to find this subject rendered in a much more lucid manner than it really is; for it consists merely of an extension of tracing on the apparent disk the path of the middle point of the visible hemisphere in mean libration, the essential principle of which, if we remember rightly, has been some years before the public. As for the mathematical treatment of the subject by such a master mind as that of Encke, and its importance in the determination of the position of objects on the moon, it is not even mentioned, so far as we could find; and we searched carefully for it. This is a subject that astronomers would certainly look for in such a work. In the chapter on the study of the surface, while the sketch of progress in mapping the moon gives a very fair idea of the successive steps in the study from the time of Galileo to the present, we miss the charm which we have always experienced when consulting the selenographical portion of 'Webb's Celestial Objects.' The description of lunar celestial phenomena is apparently too picturesque; for although we have no observational evidence of an atmosphere, it is impossible for us in our terrestrial abode to tell what effect would be produced when viewing the starry heavens from a stand-point destitute of an atmospheric covering, further than the fact that the same constellations which *we* see and which would be seen, were there eyes to see them, from all parts of the solar system, are visible from the moon. It is not difficult to ascertain the objects visible in every direction from the moon's surface; but for the greatly increased numbers of which Mr. Proctor speaks we have, as it appears to us, no real evidence. In the concluding chapter Mr. Proctor presents us with a most remarkable theory—that of the craters having been produced by meteoric impact. Many of the views set forth in connexion with the past and present condition of the surface are highly speculative and calculated to induce much thought. The question, however, arises, cannot thought be better expended on that which is real and tangible than on that which is unreal and speculative? The work itself, well conceived and in the main well executed, will afford much food for thought; and we commend it to the reader in the hope that he will examine it with the utmost care, treasure up the many truths it contains, and seek in contemporaneous and earlier literature a knowledge of those subjects which are but slightly mentioned, and which are necessary to be known in order to obtain a complete knowledge of the moon, her motions, aspects of her surface, and physical condition.

XL. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 84.]

June 19, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"Researches on Emeralds and Beryls.—Part I. On the Colouring-matter of the Emerald." By Greville Williams, F.R.S.

A considerable amount of discussion has taken place at various times regarding the cause of the colour of the emerald. Klaproth concluded from his earlier analyses that it was due to iron*; but the results of his later experiments †, made after he became aware of Vauquelin's discovery of the presence of chromium in emeralds ‡, confirmed the observations of that chemist.

From the time of Vauquelin's analyses, the colour of the emerald was always regarded as due to the presence of oxide of chromium, until the publication of the memoir of Lewy §, who, having burnt emeralds in oxygen in a similar apparatus to that employed by M. Dumas in his researches on the atomic weight of carbon, ascertained that they contained that element, and concluded that the colour was due to the presence of some organic substance. Lewy also affirmed that the deepest-tinted emeralds contained the most carbon. The small quantity of chromium contained in emeralds he considered to be insufficient to account for the colour. Wöhler and Rose ||, on the other hand, having exposed emeralds to a temperature equal to the fusing-point of copper for one hour without their losing colour, and also having fused colourless glass with minute quantities of oxide of chromium and obtained a fine green glass, considered chromium and not organic matter to be the cause of the colour.

Boussingault ¶, in the course of an investigation of the "morallons"**, arrived at the same conclusion as Wöhler and Rose; and although admitting them to contain carbon, denied that it was the cause of their colour, inasmuch as they endured heating to redness for one hour without loss of colour. This result has been confirmed by Hofmeister ††, who found that an emerald endured a red heat for hours without destruction of the colour, except at the edges,

* Klaproth, Chem. Essays, vol. i. p. 325 (London, 1801).

† Klaproth, *loc. cit.* vol. ii. p. 172 *et seq.* (1804).

‡ Vauquelin, Ann. de Chim. vol. xxvi. [1] p. 262 (1798).

§ Comptes Rendus, vol. xlv. p. 877 (1857).

|| Chem. News, vol. x. p. 22.

¶ Comptes Rendus, vol. lxix. p. 1249 (1869).

** The emeralds from the mines of New Granada are divided, according to Boussingault, into classes, two of the most important being the "canutillos," or finely crystallized, and the "morallons," or amorphous emeralds.

†† Journ. für prakt. Chem. vol. lxxvi. p. 1 (1859).

and concludes this small bleaching to arise from the destruction of the crystalline character of the stone. I have carefully repeated and extended these experiments. The emeralds employed were canutillos from Santa Fé de Bogota; they were kindly given to me by Professor Church. The following values were obtained in a determination of their specific gravity:—

Specific gravity of Emeralds (Canutillos) before fusion.

No. of experiment.	W.	W′.	t.	ρt.	D.
I.	4·4964	2·8293	16°·5	·998921	2·69
II.	4·4961	2·8294	17·0	·998841	2·69
III.	1·6655	1·0486	16·0	·999002	2·70

The formula used was

$$D=\rho t\,\frac{W}{W-W'},$$

where

W is the weight in air,
W′ the weight in water,
ρt the specific gravity of water at t°,
t the temperature of the water,
D the specific gravity.

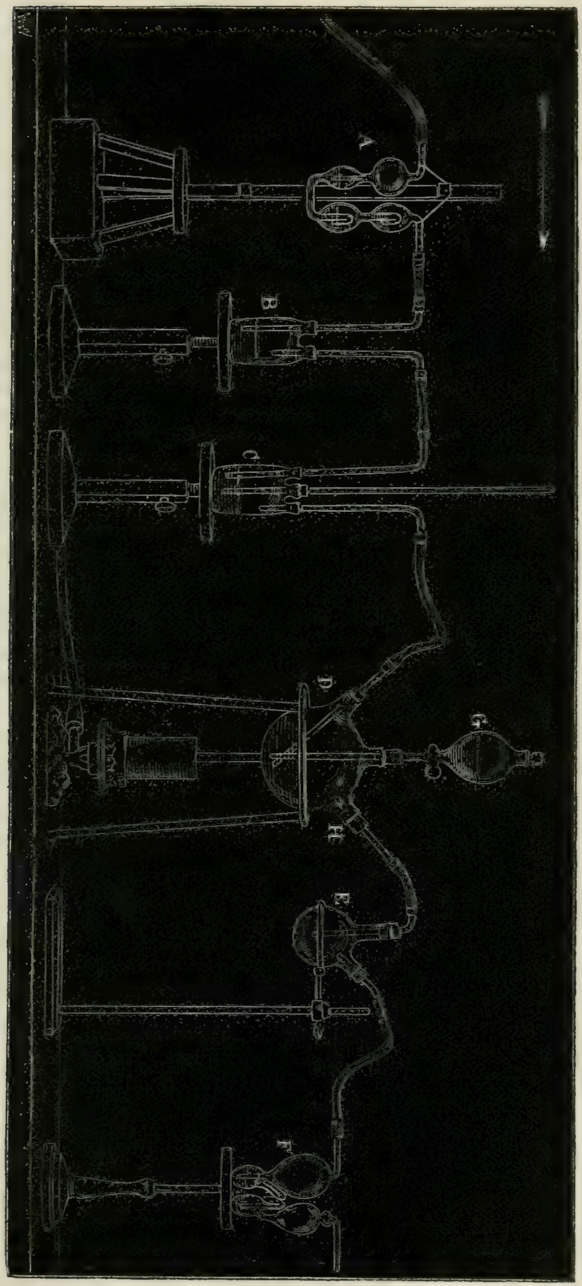
One of the above emeralds was exposed for three hours in a platinum crucible to a bright reddish-yellow heat. At the end of the operation it was rendered opaque on the edges, but the green colour was not destroyed. This experiment completely confirms those of Wöhler and Rose and Hofmeister. It is, I think, quite evident that no organic colouring-matter could withstand such a temperature for so long a time. The announcement by Lewy that the depth of colour of emeralds is in proportion to the amount of carbon present, made it at first appear improbable that colourless opaque beryls would contain any of that element. The power of the colouring-matter to resist a red heat having, however, made me inclined to disconnect the question of the colour from that of the presence of carbon, I made experiments to determine whether beryls contained that element, and, if so, to what amount. An experiment was made at this stage of the inquiry (see p. 319), the result of which showed that the beryl analyzed* contained the same amount of carbon as Lewy's emerald. As it was just possible that the small increase in weight of the potash-tubes used by Lewy, Boussingault, and myself, in determining the carbon, might not have been really due to the absorption of carbonic anhydride, but to some volatile inorganic acid produced on heating the emeralds and beryls

* As this beryl will be repeatedly alluded to in this paper, and especially in the second part, I shall, for convenience of reference, call it "beryl A." It was found in Ireland.

to redness in an atmosphere of oxygen, I felt it necessary to settle this question definitely. With this intention I burnt 1·2 grm. of beryl A in a platinum boat in a current of oxygen. The water produced was received in a U-tube filled with fragments of asbestos moistened with sulphuric acid. The carbonic anhydride was received in a Geissler's potash-tube containing lime-water; this form of potash-tube was employed in this and the other experiments to be described further on, as it enables the operator to see whether the carbonic anhydride is all absorbed in the first bulb. The carefully purified oxygen was allowed to stream through the lime-water for half an hour to prove its freedom from carbonic anhydride. At the end of that time there was no trace of turbidity. The beryl was then heated to redness; and in a few seconds the lime-water in the first bulb of the potash-apparatus became milky, thus not only proving the presence of carbon in a colourless beryl, but, taken in conjunction with the quantitative determinations, showing conclusively that the depth of colour is not, in this class of stones, in the ratio of the amount of carbon present.

But although demonstration had been obtained of the presence of carbon in the beryl A, it was still possible that it might have been derived from the decomposition of a carbonate. To settle this question, I arranged an apparatus in the following manner:—

A current of air from a gas-holder was sent in the direction indicated by the arrow (see p. 317); it passed through a solution of potassium hydrate in A and B. The three-necked bottle C contained lime-water, freshly prepared and perfectly clear. The current of air then passed into the three-necked flask D, containing 3 grms. of beryl A, finely levigated in an agate mortar, and covered to about one inch with pure distilled water. The flask E was empty, and served to arrest any thing which might have spirted over. The potash-apparatus F was filled to the height indicated with lime-water. The pipette G contained concentrated sulphuric acid. The arrangement being complete, a current of air was sent through for half an hour; not the slightest turbidity was found in C or F; the air was consequently free from carbonic anhydride. Its freedom from any other substance containing carbon had been previously determined by sending it mixed with oxygen, first into a red-hot combustion-tube, and then into a previously weighed potash-apparatus. After passage of the gas for half an hour, the potash-apparatus was reweighed and found to be absolutely unaltered. The purity of the air employed having thus been rigorously ascertained, the stopcock of the pipette G was turned, and sulphuric acid admitted into D until the water in the latter had become very hot; still no turbidity was observed in F. The fluid in D was then boiled, with the same result. It was evident, therefore, that the carbon found in the beryl was not derived from the decomposition of a carbonate. The stopper at H was then removed, and about 4 grms. of pure recently fused acid chromate of potassium were added; there was still no turbidity observed in F for twenty minutes, during all which time the fluid in D was gently boiled.



At the end of this time a cloudiness began to appear in the first bulb of F, and after half an hour in the second bulb: finally, a decided precipitate was obtained; it was collected, washed, and on analysis proved to be carbonate of calcium. The experiment was repeated in a modified manner several times. It having been found that a faint turbidity in the lime-water was sometimes obtained before the addition of the beryl, it was traced to the presence of minute quantities of organic matter in the chromate and sulphuric acid. To eliminate this source of error, the chromate and acid were mixed at the commencement of the operation, and the current of air was kept up until every trace of carbonic anhydride was removed; at this point the beryl was added, and the effect noted. The results, both with emeralds and the beryl A, however, were always precisely the same.

The apparatus was then recharged; and when half an hour's passage of the air produced no milkiness in F, 5 milligrammes of charcoal were introduced into D; in two minutes the first bulb, and in four minutes all the bulbs were rendered milky.

In another experiment, after the usual precautions, 5 milligrammes of graphite were acted on. In four minutes the first bulb, and in eight minutes all three bulbs were rendered milky.

The above experiments show, therefore, that the beryl A contains carbon, not in the state of a carbonate, but in a condition which is more slowly attacked than either free charcoal or graphite; and it is, I think, probably in the form of diamond, as has been shown to occur with the carbon contained in artificially crystallized boron*. The power of free chromic acid to attack the diamond with liberation of carbonic acid has been shown by the Messrs. Rodgers†.

The presence of carbon in beryls does not appear to be invariable. After repeated experiments upon another large beryl from Haddam County, North America, I was unable to satisfy myself that it contained carbon. It is true that traces were found in the experiment; but they were so minute that they might have been due to the difficulty of entirely excluding the presence of organic dust during the necessary manipulations‡.

The next point I wished to ascertain was the relation borne by the quantity of carbon in the beryl A to that in the emerald. For this purpose I employed a similar apparatus to that used by Dumas in his researches on the atomic weight of carbon previously alluded to. The minute error due to the apparatus was carefully determined by going through the whole process of heating the combustion-tube to redness for the same time as in the analysis, the current of oxygen passing through at the same speed, and finally

* Wöhler and Sainte-Claire Deville, Comptes Rendus, February 16, 1857.

† R. E. Rodgers and W. B. Rodgers, Chem. Gaz. vol. vi. p. 356 (1848).

‡ Since the above paragraphs were written, an interesting paper has been published by Prof. Silliman, "On the Probable Existence of Microscopic Diamonds with Zircons and Topaz in the Sands of Hydraulic Washings in California," Chem. News, vol. xxvii. p. 212.

replacing the oxygen in the system of tubes by a current of pure dry air. No appreciable error was found to affect the carbon-determination; but a correction had to be applied to the hydrogen. The necessity for minute precaution will be evident when it is considered that 1 grm. of beryl A, or emerald, only yielded 3 milligrammes of carbonic anhydride.

Estimation of Carbon and Hydrogen in Peruvian Emerald and Beryl A.

I. 0·9725 grm.	beryl A	gave	0·0030	carbonic anhydride	and 0·0131	water.
II. 1·0082	„	„	0·0031	„	0·0174	„
III. 1·1690	emerald	„	0·0030	„	0·0140	„

or, per cent.:—

	Beryl A. I.	Beryl A. II.	Emerald.	Lewy's Emerald (mean).
Carbonic anhydride	0·31	0·31	0·26	0·28
Water	1·35	1·73	1·20	1·89

In working on such minute quantities, it is not easy to speak with certainty as to the proportion of hydrogen contained in emeralds and beryls which is not due to the water present; and the difficulty is increased by the fact, insisted upon by Boussingault*, that these stones do not give off all their water below a red heat. If it be considered permissible, which I cannot admit, to calculate the hydrogen on the principle of deducting the water found on ignition in a crucible from that formed during the combustion in oxygen, and then calculating the percentage of hydrogen on the difference, as Lewy has done, the results would be as follows:—

	Beryl A. I.	Beryl A. II.	Emerald.	Lewy's Emerald (mean).
Carbon	0·08	0·08	0·07	0·08
Hydrogen	0·06	0·11		0·04

The smallness of the values obtained, and even the very fact of their close approximation, make me offer them with a certain amount of reserve; and I shall endeavour to repeat them upon much larger quantities as soon as I have found a method of avoiding all possible sources of error.

I have not inserted the numbers given by Boussingault, as there appears to be some mistake in them. He found 0·24 per cent. of carbon in the morallons, and yet says that this number agrees with two of Lewy's determinations, one of which (he says) gave 0·21, and another 0·25; whereas I find, on reference to Lewy's memoir, that that chemist obtained in the two experiments alluded to 0·21 and 0·25, not of carbon, *but carbonic anhydride.*

As it was possible that some of the carbon found in these experiments might have been derived from the steel mortar used in the preliminary crushing of the emeralds and beryls, I pulverized some emeralds in a porcelain mortar, every precaution I could devise being taken to prevent any contact of organic matter. On burning

* *Loc. cit.*

in oxygen, and passing the products of the combustion into lime-water, a copious precipitate of carbonate of calcium was obtained.

ON THE EFFECTS OF FUSION UPON EMERALDS AND BERYLS.

On the Effects of Fusion upon Opaque Beryls.—In order to study the effects of fusion upon beryls or emeralds, I found it necessary to use the oxyhydrogen blowpipe. Beryls and emeralds were amongst the numerous substances fused with this instrument by Clarke * as long ago as 1816. He states that the Siberian beryl fuses to a clear glass containing bubbles. The Peruvian emerald he found to melt very easily to a round, extremely beautiful bead free from bubbles; it lost its green colour, and became like a white sapphire.

My first experiments were made upon the beryl A; it weighed 62·54 grms., and its density was taken with great care previous to fusion. In the first experiment the whole crystal was suspended from the balance-pan, and weighed in water; in the second a few fragments were weighed in a Regnault's flask with the usual precautions. The values obtained were as follows:—

Specific Gravity of Beryl A before fusion.

No. of experiment.	W.	W′.	t.	ρt.	D.
I.	62·5400	39·0000	21°·5	·997936	2·65
II.	1·6838	1·0512	20·0	·998259	2·66

In order satisfactorily to submit beryls to the action of the oxyhydrogen blowpipe, it was necessary to find a support capable of enduring the high temperature without burning away too rapidly, and also not containing sufficient inorganic constituents to complicate the results. After a few trials I selected square prisms of gas-retort carbon, taking care to ascertain by experiment that they were adapted to the purpose. Some specimens burn away very readily; and others yield too much ash to permit of their being employed successfully. The oxyhydrogen blowpipe employed was of the ordinary construction, and, with the exception of having several nozzles of various calibres to adapt it to different quantities of the substance to be experimented upon, needs no particular description. These nozzles (which supply the oxygen) should be well formed, and free from internal irregularities. Instead of hydrogen, coal-gas was generally employed, as I have not found, for the purposes described in this paper, hydrogen to possess advantages sufficient to counterbalance the inconvenience of storing it in the large quantities required.

The phenomena observed on submitting a fragment of beryl to the action of the flame are very beautiful; but to obtain the best results, many precautions and some little practice are necessary.

* Schweigger's Journ. vol. xviii. p. 237 (1816).

The coal-gas having been lighted and the oxygen turned on, the beryl at once begins to melt, and froths or, rather, boils violently. By careful regulation of the supply of oxygen, the boiling entirely disappears; but the slightest excess of oxygen causes it to be renewed. This property of oxygen, which is found to occur with other substances besides beryls and emeralds, I hope to study in greater detail. Having so adjusted the flame that the beryl fuses tranquilly, and is yet at the exact point of maximum heat (if the substance is not too large for the apparatus), it no longer lies as a shapeless mass on the carbon support, but gathers together, rises up, and forms a perfect bead—round, clear, and brilliant. To obtain the adjustment of position necessary for this result, it is indispensable to wear very dark glasses, so dark, indeed, that objects can scarcely be discerned through them in bright daylight. Without this precaution, the minute details of the globule cannot be observed, and it would be impossible to drive away the bubbles, which form instantly when the bead is moved in the slightest degree from the proper position. The heat and glare would also soon seriously affect the sight; and, with every precaution, I have found, after the preparation of one or two hundred globules, that my sight appears (even after an interval of some months) to be slightly but decidedly deteriorated. If all is working properly, the bead should be quite mobile; and advantage of this must be taken to keep it incessantly rolling, and yet not remove it from the point where it gives out the most brilliant light. By this means the whole globule is rendered transparent. If, on the other hand, it is allowed to remain without motion on the carbon (unless the globule be very minute), it will be found, when cold, to have a white opaque base, passing into the centre of the bead in a conical form, and entirely destroying its beauty. This exact adjustment of the position of the bead at the point of maximum heat, combined with constant movement of the carbon support and perfect regulation of the proportions of oxygen and hydrogen, are indispensable to the production of the glasses, specimens of which accompany this paper.

When thus fused, the globules obtained from the beryl A were clear and colourless, but generally contained a few minute air-globules and striæ, which become obvious under the lens. Towards the end of this part of the investigation I succeeded in almost entirely avoiding these defects; but I have been compelled for a time to abandon experiments in this direction in consequence of the strain thrown upon the eyes.

When chromic oxide is added to the beads, and they are again carefully fused, they acquire a fine green colour; the tint, however, is inferior to that of the emerald. The green beads may, by an intense and prolonged heat, be rendered colourless. With cobalt oxide the beads afford beautiful blue glasses of any desired shade; and in all cases the results are the same as with the artificial mixture of beryl ingredients to be described further on.

The effect of fusion upon the beryl is to lessen the hardness and lower the specific gravity. The globules may be scratched by

quartz. The following numbers were obtained in a determination of the specific gravity:—

Specific Gravity of Beryl A after fusion.

No. of experiment.	W.	W′.	*t*.	*ρt*.	D.
I.	2·3376	1·3710	27°·2	·996603	2·41
II.	2·3376	1·3699	27·0	·996603	2·41

The beryls have, therefore, lost nine per cent. of their density in passing from the crystalline to the vitreous state.

I was desirous of carefully comparing this loss of density undergone by beryls with that of rock-crystal fused under the same circumstances. According to an experiment quoted by Forbes*, the specific gravity of quartz of undoubted aqueous origin, and also of quartz from granites, is 2·6, and that of rock-crystal, fused before the oxyhydrogen blowpipe to an amorphous glass, 2·2. According to the experiment of Le Royer and Dumas†, the specific gravity of rock-crystal determined at 4° *in vacuo* was 2·652. The value found by the Austrian Commission was 2·651223‡. I have repeated with great care the determination of the specific gravity of rock-crystal, both before and after fusion, with the annexed results:—

Specific Gravity of Rock-crystal before fusion.

No. of experiment.	W.	W′.	*t*.	*ρt*.	D.
I.	1·9493	1·2154	21°	·998047	2·65

The above number is practically identical with those of Le Royer and Dumas and the Austrian Commission. Rock-crystal fuses very readily before the oxyhydrogen blowpipe, and, if care be taken, the beads obtained are beautifully clear and free from bubbles.

Specific Gravity of Rock-crystal after fusion.

No. of experiment.	W.	W′.	*t*.	*ρt*.	D.
I.	·4116	·2240	24°	·997367	2·19
II.	·4116	·2261	25	·997120	2·21
III.	·4116	·2228	24	·997367	2·17
IV.	·3376	·1832	25	·997120	2·18
V.	·1796	·0977	11	·999655	2·19

Mean .... 2·19

Rock-crystal loses, therefore, no less than seventeen per cent. of

* Chem. Soc. Journ., new series, vol. vi. p. 225.

† Gmelin's 'Handbook of Chemistry,' vol. iii. p. 354.

‡ Ueber das Verhältniss des Bergkrystall-Kilogrammes zum Kilogramme der K. Archive zu Paris (Wien, 1870). I am indebted to Prof. W. H. Miller for this reference.

its specific gravity on passing from the crystalline to the amorphous state, or about a half per cent. less than is undergone by garnets, according to the observations of Magnus; whereas the beryl A only lost nine per cent., or little more than half as much.

On the Effects of Fusion upon Emeralds.—On heating alone before the oxyhydrogen blowpipe, these emeralds bear a bright red heat without losing their colour; and at a heat which causes incipient fusion, the edges turn colourless and opaque, while the centre remains green. After fusion for a short time they yield an opalescent greenish glass, which, kept for a long time at the maximum temperature of the blowpipe, becomes quite transparent and almost colourless. The addition of chromic oxide causes the bead to become of a dull green colour, which is not improved by moderate heating. The fact that emeralds endure a temperature capable of fusing the edges without the centre losing colour, appears conclusive against the idea of the colouring-matter being organic. The beads produced by the fusion of emeralds resemble those formed in the same manner from beryls; the phenomena during the fusion are also nearly alike; but it takes longer and a higher temperature to produce a colourless transparent bead with emeralds than with colourless beryls. The beads can be scratched by quartz; and the density is reduced to the same extent as with the beryl.

Specific Gravity of Emeralds (Canutillos) after fusion.

No. of experiment.	W.	W'.	t.	ρt.	D.
I.	·7432	·4334	13°	·999430	2·40

The density of fused emeralds is therefore almost exactly the same as the globules obtained in a similar manner from the beryl A.

Beryls, from the most various sources and of the greatest difference in appearance, vary but little in specific gravity: thus a large crystal of beryl from Haddam County, North America, weighing 1089 grms., had its specific gravity determined by suspension; and the number obtained was 2·67. The beryl A from Ireland gave 2·66; and a beautiful transparent yellow crystal, the locality of which is doubtful, gave 2·69, or exactly the same as the emeralds from Santa Fé.

On the Effects of Fusion upon an Artificial Mixture of Beryl Ingredients.—Being desirous of trying the effects of fusion upon an artificial mixture of the same composition as that of a beryl, I made a series of careful analyses of the beryl A. The results of these analyses have led me to a laborious examination of the processes at present in use for the separation of alumina from glucina. The study of the original carbonate-of-ammonia process of Vauquelin, and the modifications of Rose, Joy, Hofmeister, and others, has taken twelve months of constant work; but even my earlier analyses enabled me to obtain a sufficiently close approximation to the composition of the beryl A. The following were the

proportions used :—

Silica	67·5
Alumina	18·5
Glucina	14·0
	100·0

I did not introduce any iron or magnesia, as I regard them as accidental impurities varying in amount.

When a mixture of the above composition is exposed to the flame of the oxyhydrogen blowpipe, it fuses with almost exactly the same phenomena as with the natural beryl. It is, however, as might be anticipated from the absence of iron and chromium, much easier to get a colourless transparent bead with the mixture than with either emeralds or beryls. The greatest difficulty in this respect is, of course, found with emeralds. The specific gravity of the fused globules was determined, with the following result :—

Specific Gravity of Artificial Amorphous Beryls.

No. of experiment.	W.	W′.	t.	ρt.	D.
I.	·5774	·3394	13°	·999430	2·42

or almost exactly the same as the density of native emeralds and beryls after fusion.

When a small portion of chromic oxide is added to the artificial mixture and the whole is subjected to fusion, the resulting bead is of a rich yellowish green, and in many experiments approached to the emerald tint ; but, as a rule, the colour is more of a faded leaf-green ; and although I have never obtained a globule of the vivid tint of a fine emerald, the glasses, when well cut, are quite beautiful enough to serve as jewels. Prolonged heating gradually diminishes the colour, the bead gradually becoming of the palest bottle-green, and, finally, nearly colourless. This result is the same as with the emerald.

The metallic oxide which yields the finest tints when fused with opaque beryls, or the artificial mixture, is that of cobalt. The manner in which this oxide withstands the intense heat of the oxyhydrogen flame is remarkable. All tints, from nearly black to that of the palest sapphire, can be obtained ; and the resulting glasses, when cut, are extremely beautiful, and have almost the lustre of crystallized gems.

The globules obtained by fusing the artificial mixture of beryl ingredients with didymium oxide show the characteristic absorption-spectrum of that metal in a very perfect manner, the lines being intensely black. Even when the bead is quite opalescent from insufficient heating, the black lines are beautifully distinct in the spectroscope. With a large quantity of didymium oxide the beads are of a lively pink, becoming more intense by artificial light, and, when cut, form very pretty gems. The presence of didymium in sufficient quantity raises the specific gravity.

Specific Gravity of Artificial Amorphous Beryls containing Didymium.

No. of experiment.	W.	W′.	t.	ρt.	D.
I.	·9467	·5815	11°	·999655	2·59

the resulting number being almost as high as that of the emerald before fusion.

Conclusions.—The evidence given in this paper, showing that colourless beryls may contain as much carbon as the richest-tinted emerald, taken in conjunction with the ignition experiments, and the results of the fusion of chromic oxide with colourless beryls and with an artificial mixture of the same composition, leaves me no room to doubt the correctness of Vauquelin's conclusion, that the green colour of the emerald is due to the presence of chromic oxide.

The fact that emeralds and beryls lose density when fused cannot properly be cited as proving that they have been made in nature at a low temperature; for it is quite possible that they were crystallized out of a solution in a fused mass, originally formed at a temperature high enough to keep the constituents of the emerald in a state of fusion, and that the crystals developed themselves during a slow process of cooling or evaporation. The method employed by Ebelmen* for the artificial production of chrysoberyl, namely heating alumina, glucina, and carbonate of calcium with boracic acid in a porcelain furnace until a portion of the menstruum had evaporated, yielded crystals of the true specific gravity, showing the density of minerals to be less dependent on the temperature at which they are produced than upon their crystalline or amorphous state.

One crystalline gem (the ruby) has undoubtedly been produced in nature at a high temperature. I have frequently repeated Gaudin's† experiment on the artificial formation of this stone, and can confirm most of his results. I did not, however, find the density to be quite the same as that of the native ruby or sapphire, which is, in different specimens, from 3·53 to 3·56. Artificial rubies of the finest colour made by me by Gaudin's process had a specific gravity of 3·45, which is not three per cent. lower than that of the ruby. The reason for this close approximation will be found in the fact that fused alumina crystallizes on cooling. The crystallization, however, is confused and imperfect, which causes the resulting product to be only partially transparent, and to have a slightly lower specific gravity than the natural gem. It is, consequently, scarcely correct to call the fused stones made by Gaudin's process "artificial rubies."

I have convinced myself that rubies have been formed in nature at a temperature equal, or nearly equal, to that of the fusing-point of alumina, from the circumstance that the reaction between chromic

* Ann. Chim. Phys. [3] vol. xxii. p. 223 (1848); vol. xxxiii. p. 40 (1851).
† Ann. Pharm. vol. xxiii. p. 234.

oxide and alumina, which results in the development of the red colour of the gem, is not effected at low or even moderately high temperatures, but requires a heat as high as that of the oxyhydrogen blowpipe. It is not necessary that the chromium should be presented to the alumina in the form of chromic acid. It appears, therefore, that the red colour of the ruby is not caused by the presence of chromic *acid*; it is, in fact, a reaction *sui generis* between alumina and chromic oxide, which, as far as my experiments have gone, only takes place at very elevated temperatures.

In my next communication I propose to give the results of a comparative study of two of the processes most generally employed for the analysis of emeralds, beryls, and other minerals containing glucina and alumina—namely, the carbonate-of-ammonia process of Vauquelin, and the caustic-potash method devised by the same chemist, but modified by Gmelin, and generally associated with his name. These studies are already far advanced.

Specimens of various beryl glasses, cut and uncut, accompany this paper.

GEOLOGICAL SOCIETY.

[Continued from p. 174.]

January 8, 1873.—Prof. Ramsay, F.R.S., Vice-President, in the Chair.

The following communication was read:—

"The Secondary Rocks of Scotland."—First Paper. By John W. Judd, Esq., F.G.S.

Introduction.

The Mesozoic periods are in Scotland represented only by a number of isolated patches of strata situated in the Highlands and Western Isles, which have been preserved from the destructive effects of denudation either through having been let down by great faults among the Palæozoic rocks, or through being sealed up under vast masses of Tertiary lavas. The Cretaceous rocks, exhibiting very interesting characters and yielding a beautiful series of fossils, were discovered by the author of the paper during the past year on the mainland and in several of the islands of the west of Scotland. The Jurassic rocks, which were first described by Murchison, are now shown to present a remarkable contrast with their equivalents in England, in being constituted, *throughout their whole thickness, by alternations of marine and estuarine series of beds,* in which respect they precisely resemble the equivalent strata of Sweden. The Triassic rocks have now been discovered in Sutherland, where their conformable relations to overlying beds containing a fine Liassic fauna, entirely confirm the conclusions concerning their age derived from Prof. Huxley's studies of the remarkable reptiles yielded by them in Elgin.

Part I. *Strata of the Eastern Coast.*

These consist of a number of patches, situated around the shores

of the Moray Firth, in the counties of Caithness, Sutherland, Ross, Cromarty, and Elgin. The preservation of these is shown to be entirely due to the operation of faults of enormous magnitude, which have let down the Mesozoic strata against the various Palæozoic rocks. Owing to an insufficient examination of the palæontological evidence, much misconception has hitherto prevailed concerning the geological age of most of these patches; but a careful study of their faunas enables us to reconstruct an almost unbroken history of the Triassic and Jurassic periods in the north of Scotland; while even concerning the Cretaceous much important evidence is afforded by the boulders, which abound in the drifts.

The Keuper is represented by the Reptiliferous Sandstone and the overlying calcareous rocks; the Rhætic by conglomerates in Sutherland, and probably elsewhere by estuarine beds, which are now, however, only preserved in great boulders.

The Lower Lias is constituted in its lower part by a thick series of estuarine beds, sandstones, shales and coals (hitherto referred to the Lower Oolites), and in its upper part by marine strata yielding a highly characteristic fauna. The Middle Lias is formed of clays seen *in situ* in Sutherland, and micaceous sandstones, only preserved in boulders; both furnish very fine series of fossils. The Upper Lias is probably represented by estuarine beds.

The Lower Oolites are almost wholly made up of estuarine strata, containing coal-seams, which have been frequently worked in the past in mines which are now being reopened. The beds yield many freshwater fossils, which have attracted much attention, owing to the general resemblance they present to those of the Wealden.

Of the Middle Oolites we have a wonderfully complete series in the east of Scotland. At the base is a calcareous sandstone crowded with Kelloway fossils; above this 300 feet of shales, yielding the well-known Ammonites and other fossils of the "Ornatus-clays" (Middle Oxfordian); still higher are beds of marine sandstone with a magnificent fauna, identical with that of our Lower Calcareous Grit. A thickness of 400 feet of estuarine sandstones &c., which covers the last, is surmounted by limestones, shales, and sandstones, with the fossils of the Coral Rag.

The Upper Oolites, now for the first time recognized in Scotland, are of great thickness, and consist of alternations of estuarine and marine strata, yielding a splended fauna and flora. In their northern extension these beds pass into the wonderful "brecciated beds" of the Ord, which contain enormous transported blocks of Old Red Sandstone. The features presented by these strata are of the highest interest, and are very suggestive of the prevalence of peculiar physical conditions in the area towards the close of the Jurassic period.

The patches of Secondary strata on the coasts of Ross, usually called Lias, are shown to belong to the Lower, Middle, and Upper Oolites; and a mass on the coast of Elgin is demonstrated to be also of Oolitic age.

In future papers the author proposes to describe the Secondary rocks of the west coast and islands of Scotland, and to discuss the

various theoretical questions suggested by a comparative study of the whole of the Scottish Mesozoic strata.

January 22, 1873.—His Grace the Duke of Argyll, K.T., F.R.S., President, in the Chair.

The following communication was read:—

"On the Glaciation of Ireland." By J. F. Campbell, Esq., F.G.S.

The author stated that almost the whole of the surface of Ireland consists of glaciated rocks less or more weathered, or well preserved. The polished surfaces are covered in low grounds with drift. Boulder-clay, unstratified, is next to the rock; sands and gravels and peat bogs are above the clay. The solid rocks have been greatly worn away since the formation of the Antrim basalt; the drift since the Glacial period. The hills and hollows in the rocks are the result of wearing and "denudation;" the débris is the "drift" partially rearranged.

This was shown by examples in:—1st, chalk and basalt in Antrim; 2nd, mountain-limestone &c. in Sligo; 3, older rocks about Valentia and the south-west; 4, granites and metamorphic rocks in Donegal and the north-east. The effect of the Atlantic on cliffs at Slieve Liag in Donegal, and elsewhere on the Irish coast, was noticed.

It was shown from these large coast-sections that the upper surface now has no relation to the older contortion, fracture, and folding of these disturbed and faulted rocks, which lie under newer and less-crumpled beds, up to the peat. The probable dimensions of the ice-engines which worked on the surface of Ireland was shown by comparison of glaciers in Iceland, Norway, and elsewhere, with the Irish marks, which indicate ice of equal size. Beginning with the smallest and rising to larger systems, Irish marks indicate ice of equal dimensions, till horizontal grooves at 2000 feet above the sea indicate ice more than 2000 feet thick, moving over Ireland into the Atlantic in a south-westerly direction. It was shown that the ice at its maximum probably extended from the Polar Basin to Cape Clear. In support of this view, boulders on Fairhead, and the denudation and glaciation of the central Highlands of Scotland, and of Scandinavia, Finland, and the United States, were shortly noticed. The question whether these extensive tracks were made by glaciers or by icebergs was discussed. The marks in Ireland and Scotland seem to the writer to indicate ice more than 2000 feet thick moving along the bottom of lochs, straits, and shallow seas, in water less than 1800 feet deep, with large local ice-systems upon high lands. It was shown that glacier-ice aground in water is easier to push horizontally, and so to drive over impediments, in proportion to the weight lifted vertically by flotation.

Rubbings from glaciated rocks placed beside shaded Ordnance maps of parts of Scotland and England, showed that similar forms had been somehow produced on scales of inches and miles upon the rock-surface of Scotland.

The author's conclusion is as follows:—

"Ireland has been greatly denuded. Glacial and marine action

are the most powerful known to me. Glaciers and the sea shaped Ireland, as I believe. Rivers and weathering have done little to obliterate the tool-marks of ice and the sea since the end of the last of a series of Glacial periods."

XLI. *Intelligence and Miscellaneous Articles.*

ON SOLUTION OF NITRATE OF NICKEL AS AN ABSORPTION-PREPARATION. BY DR. H. EMSMANN.

AMONG the colour-spectra exhibited by coloured liquids, that of the beautiful apple-green solution of nickel in nitric acid appears to me especially worthy of notice. Having filled a hollow prism with this solution, I found that the terminal colours (red and violet) in the spectrum were absorbed. This was new to me; but in Mousson's *Physik auf Grundlage der Erfahrung* this peculiarity of the solution of nitrate of nickel is cited as a thing known.

Now, while with most coloured liquids the colour results as a mixture of all the spectral colours under the predominance of that of the substance in question, we have here the beautiful green as a mixture of the spectral colours with the exception of the red and the violet. This fluid is therefore particularly well adapted and convenient, not merely for showing in the course of instruction the phenomena of absorption of colours, but also for ascertaining the composition of the colours of substances.

I keep, for the purpose of instruction, some of this solution ready in a glass phial, the sides of which are as nearly as possible parallel (I obtained such a one at a perfumer's), and use it for demonstrating the absorption. On pasteboard covered with black paper narrow strips of coloured paper are pasted, among which are different reds. One sort of red is not to be perceived through the liquid; a second appears dark blue; a third, yellow. A violet stripe is likewise not to be perceived; a white stripe appears green. Titles of books, printed in red, looked at through the liquid appear dark; and so do the red and violet parts on woollen embroidery.

In most text-books of physics the phenomena of absorption have till now been for the most part only briefly touched upon. Wüllner's *Lehrbuch der Experimentalphysik* forms an honourable exception, since in it are given not only the different methods of investigation, but also several examples of body-colours, with the account of the colours from the mixture of which they result. Coloured liquids generally seem to have been but little investigated. Mousson cites didymium, chlorophyl, and blood. A comparison of the spectra of coloured liquids would be very desirable. Solution of sulphate of copper shows particularly violet, yellow, and blue in the spectrum, in about the proportion 7 : 5 : 2, and some red. In the spectrum of sulphate of iron, green predominates. Ferrocyanide of potassium gives red, green, violet, and dark blue.

The purpose of this note is chiefly to call the attention of my colleagues to the solution of nitrate of nickel, and to recommend

its introduction into physical cabinets.—Poggendorff's *Annalen, Ergänzung*, vol. vi. pp. 334, 335.

DETERMINATION OF THE FRICTION RESISTANCES IN ATWOOD'S MACHINE. BY C. BENDER.

The determination of the amount of friction in the wheel of the fall-machine is preceded by the determination of its moment of inertia. This can be done by applying a small overweight p to one of the two suspension-weights, determining the acceleration γ hereby produced, and the unknown moment of inertia from the formula

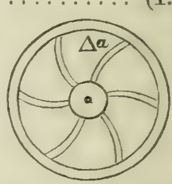
$$\gamma = \frac{gp}{2P+k+p},$$

wherein P denotes the suspension-weight, equal on both sides, and g the acceleration of gravity. It is understood that for P and p in this formula their masses must be put.

The above method cannot give exact results, since in it no account is taken of the friction of the pin in the wheel; and it is especially useless where the machine worked with has no friction-rollers. Also the method according to which the moment of inertia of a composite body is calculated from that of its separate parts, referred to one and the same axis, cannot be employed here, because the constituent parts of such a wheel but rarely permit a convenient determination of the moment of inertia.

Very convenient and accurate is the method which relies on the oscillations of a material pendulum. If k denotes the moment of inertia of a pendulum in reference to its axis of rotation, M its mass, and a the distance of the centre of gravity from the rotation-axis, the oscillation-period of such a pendulum is expressed by

$$t = \pi \sqrt{\frac{1}{g} \cdot \frac{k}{Ma}} \quad \text{(I.)}$$



In an experiment the wheel of the machine was placed on a horizontal smooth edge of wood (or of steel), at the point a (see the annexed figure); and then the duration of an oscillation was determined very accurately. The experimental data were:—

Weight of the wheel	178·9 grammes.
Diameter of the wheel (string-groove)	12 centims.
Distance of the rotation-axis a from the centre of gravity	5·175 centims.
200 vibrations in	60·5 seconds.

From formula I. there results

$$k = 8·5836.$$

According to a theorem of mechanics, the moment of inertia of a body, referred to any axis, is equal to its moment of inertia in reference to an axis through the centre of gravity parallel to the former, added to the product of the mass of the body into the square

of the distance between the two axes. We will obtain according to this theorem the moment of inertia k' of the wheel in reference to the axis through its centre of gravity:—

$$k'=8\cdot5836-\frac{178\cdot9}{981}\,.\,5\cdot175^2;$$

$$k'=3\cdot70.$$

The weight of the heavy mass which at the circumference of a wheel opposes to a turning force the same resistance as the mass 3·7 at the unit of distance from the rotation-axis which passes through the centre of gravity is now found from $\frac{3\cdot7\,.\,981}{6^2}=100\cdot59$ grms.; and this is the amount which must be brought into the reckoning in determining the acceleration in the Atwood machine.

If in experimenting with the machine we take away the overweight after the motion has continued a certain time, the entire system must, in consequence of its inertia, move further with a uniform velocity, which depends on the time or the height of the fall and on the acceleration which has been present. Such a uniform motion, however, will be prevented by the friction of the axle of the wheel, introducing a uniformly retarding motion which, after a time, brings the system to rest. If we regard the friction present as a constant force, under the influence of which a moving body attains the acceleration μ, and assume that the motion is produced by an overweight of the mass m and has extended, at the removal of the overweight, to the distance h, in this case the velocity attained is

$$=\sqrt{2g\frac{m}{\text{M}+m}\,.\,h}-\sqrt{2\mu h},$$

where $\text{M}+m$ denotes the entire mass set in motion, and g the acceleration produced by gravity. If from the removal of the overweight to the resting-point the system advances the distance σ, the calculation of the velocity mentioned can be arrived at by assuming that any body whatever has, under the influence of the acceleration μ, travelled the distance σ. This gives the equation

$$\sqrt{\frac{2gm}{\text{M}+m}\,.\,h}-\sqrt{2\mu h}=\sqrt{2\mu\sigma}$$

or

$$\mu=\frac{gmh}{(\text{M}+m)(\sqrt{\sigma}+\sqrt{h})^2}. \quad \ldots\ldots\ldots\ldots \quad \text{(II.)}$$

For this μ we can also form another expression; for if x denote the mass (or the weight of the mass) which counterbalances the influence of the friction, we have as a second equation

$$\mu=\frac{g\,.\,x}{\text{M}+m+x}. \quad \ldots\ldots\ldots\ldots\ldots \quad \text{(III.)}$$

Before I proceeded to the determination of x, I satisfied myself whether the quantity σ in equation II. could be ascertained with

sufficient certainty, and especially whether the quantity μ proves to be independent of h, equation II. For this end three experiments were carried out, in which

$$m=2\cdot2 \text{ grms.}$$

$$M+m=300 \text{ grms.}$$

$$g=981 \text{ centims.}$$

The results were:—

For $h=28$ centims.

1. $\sigma=112, =112\cdot3$, and $=112\cdot8$ centims.; mean$=112\cdot3$ centims.

For $h=25$ centims.

2. $\sigma=86\cdot6$ or $86\cdot8$ or $87\cdot2$; mean $=87$ centims.

For $h=23$ centims.

3. $\sigma=81$; mean $=81$.

According to formula II., from 1, $\mu=0\cdot80$ centim.; from 2, $\mu=0\cdot88$ centim.; from 3, $\mu=0\cdot88$ centim.

In further experiments the amount of the friction itself was ascertained. According to the arrangement of the machine, 24 inches descent $=65$ centims.; therefore $362\cdot21$ inches corresponded to the acceleration of gravity. The overweight $m=1\cdot514$ was chosen so that $\frac{m}{M+m}=\frac{1}{150}$, and consequently $\frac{gm}{M+m}=2\cdot415$ inches fall.

With $h=9$ inches, $\sigma=25$ inches fall, and $2\mu=\frac{21\cdot732}{64}$.

From equation III., x was now found $=0\cdot21$ grm. We will call this weight the *friction-weight.*

In another experiment the friction of the pin of the wheel in its socket was increased by means of screws; and there resulted, under otherwise like circumstances, for $h=9$, $\sigma=16\cdot5$ inches fall, $x=0\cdot300$.

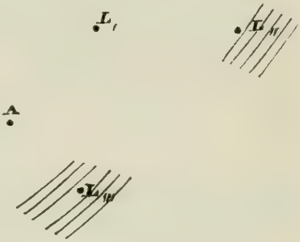
Both experiments were in perfect accord with the calculation. The friction-weights I made of tinfoil, of the form of the usual fall-weights.

In order to make use of the method of friction-determination conveniently, it will be prudent to construct a Table, and to calculate the friction-weights with equal $g\cdot\frac{m}{M+m}$ and h within certain limits of σ. For various reasons it is unnecessary to actually draw up such a Table here.—Poggendorff's *Annalen*, vol. cxlix. pp. 122-126.

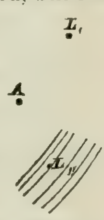
A REMARKABLE INTERFERENCE-PHENOMENON OBSERVED BY M. SEKULIĆ.

I was sitting one evening opposite to a looking-glass. Happening to cast a glance into it, I noticed on my head splendidly coloured interference-streaks. On more attentively considering the circumstances which might occasion them, I remarked in the mirror three

bright images—those of two petroleum-lamps and one stearine candle. As tobacco was being smoked in the room, I at first believed that the smoke had produced a similar phenomenon as slightly moist air when it forms a halo round the moon or the sun, and that in consequence of this the appearance was seen. I could not, however, perceive those halos round the flames, either in the looking-glass or in the air. Abandoning the attempt to fathom the cause, I occupied myself with the phenomenon itself, and perfectly recognized it as a phenomenon of interference, because the streaks widened as the angle of reflection increased. In the observation the flame-images and the image of the eye had the following position :—



A is the image of the eye, $L_{,}$ that of the petroleum-flame before the looking-glass, $L_{,,}$ that of a second petroleum-flame at a greater distance, and $L_{,,,}$ the image of the candle burning on the table. The interference-streaks at $L_{,,}$ and $L_{,,,}$ are represented in their situation. As the phenomenon was observed in a public place, where there were many other glasses and lights, I could not pursue the matter further; but I resolved to repeat the experiment at home or in my study. To this end, I placed opposite to the looking-glass in my room, at about 6 metres distance, a petroleum-lamp; I then took a stearine candle in my hand, and so endeavoured to get the position which I had had in the first observation. After looking from several different points, and shifting the candle several times, I at length remarked faint streaks round my chin; on further moving backwards and forwards, the streaks were very beautifully developed, but not so brilliant as on the first occasion. I now noted the position. $L_{,}$ was the light of the petroleum-lamp, $L_{,,}$ that of the candle, and A the eye (of course seen in the glass). The streaks attained the greatest intensity when $L_{,,}$ was situated exactly under $L_{,}$, and A was to be seen a little sidewards between the two. The streaks widened out above $L_{,,}$—not nearly as about a slit, but quite uniformly around the flame, so that it seemed to have no further influence on the streaks than to illuminate them. The flames were now left in their position, the glass taken down and carefully wiped and hung up again in the same place. The streaks were again visible. I had then carefully to investigate whether dampness adhering to the glass, or particles of dust, called forth the phenomenon; for I could not derive it from the laws of refraction and reflection in the clean mirror. I next dusted the mirror with lycopodium in such manner that around the light-images splendid halos appeared. The marked position was then taken, from which gorgeous interference-streaks were to be seen; nay, they arose not merely around the lower, but also around



the upper flames; and since these must cross one another, the result was a coloured picture of crossing waves, than which none more beautiful can be imagined. It was exactly, in form, like that which ensues when two stones are thrown upon the surface of still water; but it is distinguished by the magnificence of its colours. I publish this notice preliminarily, in order that this simple and very instructive experiment may be repeated; I intend, however, to subject the phenomenon to scientific investigation, because the few experiments I have at present made have led me to the conviction that, besides dust-particles, the hygroscopic moisture adhering to glass, though ever so little, is capable of producing it. I believe, further, that the degree of moisture may be inferred from the intensity of the streaks. Though, perhaps, of little value in relation to meteorology, it will incontestably give to many a physicist a good test for the degree of dryness of his glasses.

The results of my further investigations I shall likewise communicate to the *Annalen*.—Poggendorff's *Annalen*, vol. cxlix. pp. 126–128.

NOTE ON THE FILLING OF VESSELS WITH A VERY NARROW TUBE, ESPECIALLY THE CARTESIAN DIVER. BY K. L. BAUER, OF KARLSRUHE.

Frick (*Physicalische Technik*) and Weinhold (*Vorschule der Experimentalphysik*) recommend, in order to introduce a sufficient quantity of water into a Cartesian diver (best consisting of a hollow glass ball with a narrow tube), to partially expel the air by heating, and then to dip the open tube into water. For this procedure, however, or the use of the air-pump for the same purpose, another method, very simple, can be substituted as follows:—

1. Fill the cylinder intended for the diver-experiments with water; insert the *empty* swimmer in the usual position, in which the tube is directed downwards, and close the cylinder with a caoutchouc cap and binding-thread.

2. Exert a strong pressure on the elastic covering, and, *continuing the pressure*, *incline the cylinder* to the horizon so that the caoutchouc cap is perceptibly *lower* than the foot of the cylinder, and the diver, with the tube directed obliquely upwards, begins to *ascend*. At this moment, or when the swimmer has arrived at the top, remit the pressure, and air-bubbles will escape from the ball, which is now *partly filled with water*. If the cylinder had been completely inverted, so that in its new vertical position the cap had been quite under the bottom of the cylinder, the diver would have ascended with great velocity, especially if the pressure had been interrupted immediately after the inversion.

3. If the cylinder be now restored to its original erect position (when the swimmer also returns to his initial place), and the above process be repeated, one very soon succeeds in filling into the diver so much liquid that, even after complete inversion of the cylinder, he will not ascend at all if strong pressure be continuously applied

to the cap, and only very slowly if the pressure be relaxed. The Cartesian diver is now suitable for its ordinary use.

4. After reiterated repetition of the given procedure, the relaxation of the pressure is useless; the diver is filled to such a degree that he remains below when the cylinder is turned upside down, the ball resting on the cap, and the tube pointing upwards.

5. If the cylinder be kept inverted, and the cap be pressed several times in quick succession, at each interruption of the pressure an air-bubble escapes, by which the diver becomes yet more completely filled. At last these means are no longer operative; a minute air-bubble remains in the ball, but is of no import for its use as a diver, since the requisite degree of filling has been *exceeded*.

After the diver has been taken out of the cylinder, the water which has entered can be removed by a vigorous shaking, of course with the tube downwards.—Poggendorff's *Annalen*, *Ergänzung*, vol. vi. pp. 332, 333.

RESEARCHES ON THE SPECTRUM OF CHLOROPHYL. BY J. CHAUTARD.

Conclusions.—I. The spectrum of chlorophyl is characterized by certain bands, among which there is one, in the red, the special properties of which suffice to distinguish the solution. The qualities of this band are *sensibility*, *certainty*, and *generality*.

a. Sensibility, by clear outlines, a fixed position, and a remarkable permanence through a solution so dilute as to contain less than $\frac{1}{10000}$.

b. Certainty, by the division into two which it undergoes under the influence of the alkalies—a character which belongs neither to the lines of blood, nor to those of bile, nor to those of any other organic fluid.

c. Generality; that is to say, this line always appears wherever chlorophyl exists, pure or adulterated.

II. Chlorophyl exists, in vegetables, in three different states, which can be perfectly recognized in the spectroscope—in leaves newly formed, in adult leaves, and in leaves dead or detached from the vegetable.

a. In young leaves just expanding, the instability of the elements is very great, and is recognized by the appearance of *temporary accidental* bands under the action of chlorhydric acid.

b. In the second case the same acid gives rise, in the alcoholic solution, to quite another system of bands, which I call *permanent accidental* bands.

c. Lastly, In alcoholic solutions of leaves from which the life has disappeared, or in those of fresh chlorophyl which have undergone a certain amount of change, the permanent accidental bands appear immediately, *without the intervention* of chlorhydric acid.

III. As the last consequence of the spectrum-analysis of chlorophyl, let us say that this substance, so easy to modify when considered in the physiological point of view, is nevertheless much less

alterable than it is generally believed to be. It resists the action of iodine, the acids, the alkalies, and the process of digestion, and retains, under the influence of these agents, if not its primitive composition and properties, at least some characters which permit its recognition in the midst of the most complex and varied mixtures and after a considerable lapse of time.

If the alcoholic tincture is destroyed pretty rapidly when exposed to the air, and especially in sunshine, the oily solutions oppose to the same agents a very remarkable resistance, of which we have set forth the consequences in certain questions of natural philosophy. —*Comptes Rendus de l'Académie des Sciences,* Sept. 8, 1873.

ON THE DIRECT SYNTHESIS OF AMMONIA. BY W. F. DONKIN.

The action of induced electricity on mixtures of certain gases has been lately shown by Sir Benjamin Brodie (Proc. Roy. Soc. April 3, 1873) to yield very interesting results.

An obvious application of his method was to treat a mixture of dry hydrogen and nitrogen in a similar manner as those referred to above, with the view of effecting the synthesis of ammonia; and Sir B. Brodie kindly allowed me the use of his apparatus for the purpose of the experiment, which was conducted as follows:—

A mixture of about three volumes of hydrogen with one of nitrogen in a bell-jar over water, was passed through two tubes containing pumice moistened with alkaline pyrogallate and sulphuric acid respectively, then through a Siemens induction-tube, and into a bulb containing dilute hydrochloric acid. The whole apparatus being first filled with pure hydrogen, about half a litre of the mixed gases was sent through the apparatus, the induction-coil not being in action; the bulb containing the acid was then removed and another substituted, containing an equal volume of the same acid.

About half a litre of the mixed gases was now passed through the apparatus, submitting them to the action of the electricity. The contents of the two bulbs were next transferred to two test-tubes; and after adding excess of potash to each, Nessler's test was applied. The first solution gave a faint yellow coloration, the second a rather thick reddish-brown precipitate.

No attempt was made to estimate the quantity of ammonia formed, as it would vary with many of the conditions of the experiment.

Since writing the account of the above experiment, which was made in Dr. Odling's laboratory at Oxford on March 24, I have seen in the *Comptes Rendus* for April 22, 1873, a note of an experiment by Messrs. Thénard of Paris, in which they observe the formation of traces of ammonia by the action of electricity on a mixture of hydrogen and nitrogen; but no details of the mode of operating are given.—*From the Proceedings of the Royal Society,* May 1, 1873.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

NOVEMBER 1873.

XLII. *On the Action of a Blast of Sand in cutting hard Material.* *By* Professor OSBORNE REYNOLDS, *M.A.**

MR. TILGHMAN has discovered that sand will, when blown against any hard body, rapidly wear it away—that it acts in what may be called a systematic and not merely in a casual manner (that is to say, each grain produces its effect, and not merely one grain here and there just scratches the surface)—that the material of the sand is not necessarily harder than the material acted upon, but that the hardest bodies, such as steel files and diamond, may be cut by common sand or even by small leaden shot—that it is not necessary that the grains of sand should be of any particular size, the smaller grains producing apparently the same effect as the larger—and that the velocity with which the sand must be driven is after all nothing very considerable, that for glass a velocity of 50 feet per second is sufficient.

Now these facts, in addition to their value from a useful point of view, serve a scientific purpose, inasmuch as they call our attention to circumstances of impact which have hitherto passed unnoticed. When once our attention is called to them, we can see that these circumstances have been before us in a variety of shapes, and that they play a very important part in many mechanical phenomena, but never till now have they taken a shape sufficiently definite to compel attention.

That one hard body will, when driven with sufficient velocity,

* Communicated by the Author, having been read before Section A of the British Association, September 1873.

penetrate or fracture the body it strikes, or will itself be fractured, is a matter of such common experience, and may in some cases be so simply explained, that we have so far contented ourselves with the simple explanation, and have not endeavoured to distinguish between the different cases which occur. Up to the present time the laws of impact have been considered mainly with reference to the motion of the centres of gravity of the bodies, and very little attention has been paid to what actually occurs at the point of contact. Hence it is that the fact which it is my object in this paper to point out has been so far overlooked. This is, *that at the instant of first contact the pressure between the bodies is independent of their size, and depends on the density of the body as well as on the hardness, so that a heavy soft body may cause as much pressure as a hard body.*

In the very striking process of the sand-blast we see at a glance evidence of two things which are contrary to our conception of what ought to be; that is, (*a*) the effect is independent of the size of the particles, the small ones acting as soon as the large ones, and (*b*) the softer bodies penetrating the harder, as in the case of soft leaden shot penetrating the hard glass. In order to explain these we must consider what actually takes place at the point of contact.

(*a*) *The intensity of the pressure between bodies on first impact is independent of the size of the bodies.*

Although it is customary in mathematical works to consider the effect of impact on the masses of the bodies as instantaneous, we all know that it is not really so. Time is occupied in changing the motion of the centres of gravity of the bodies. The idea of a sudden change may, however, have arisen from confusion between what happens at the point of contact and at the centre of gravity. At the point of contact it is clear that the effect on the motion of the surfaces of the bodies must be instantaneous. After impact the surfaces of the two bodies move together; hence the motion of one or both of them must be instantaneously altered*. This effect, however, as will be presently proved, is in the first instance confined to the particles which come into contact. Even with hard bodies such as glass it takes time for the pressure to extend into the body. Hence on first contact it is only those particles in contact which are affected, and the rest of the body might be removed without altering the effect; that is to say, the first effect of the impact will be independent of the quantity of material behind the particles which actually impinge.

* This first occurred to the author while reading a paper "On the Rupture of Iron Wire by a Blow," by John Hopkinson, read before the Manchester Literary and Philosophical Society, 1871-72.

(*b*) *A soft body may cause pressure sufficient to crush a hard body.*

The pressure necessary to force-in (to indent) the surfaces at the point of contact will depend on two things—(1) the inertia of the particles at the surface, (2) the support these particles receive immediately from behind.

1. The inertia will depend simply on the density; and if we regard all bodies as liquids, we may express this part of the pressure as similar to that of a jet against a surface. With liquid or very soft bodies this will be the only source of pressure. And in the case of hard bodies it seems probable that this part of the pressure will depend on the same laws as for liquid bodies; for there is close analogy between the flow of solids under pressure and that of liquids.

2. The support which the surface receives from behind will depend on the elasticity of the material and on the density. In the case of hard bodies such as glass, this will constitute almost the entire source of pressure. If the body were compressible in the same sense as gas, it would be the only source of pressure; but in the case of elastic but incompressible bodies the inertia will affect the pressure, and it is assumed that it affects it in the same way as in the case of a liquid.

When a soft body such as lead strikes a hard surface such as glass, if the velocity is sufficient, the inertia of the lead will cause enough pressure to crush or penetrate the glass, however small the projectile may be.

It now remains to show more definitely how the pressure is connected with the velocity and nature of the bodies in impact.

In doing this I shall assume, first, that the bodies are elastic, and, second, that they are incompressible fluids.

By elastic bodies are meant bodies compressible in the same sense as gas is compressible; ordinary bodies are not compressible, and any calculations founded on their being so are necessarily erroneous. If, however, the bodies are first treated as elastic and then as fluid, and the results compounded, we shall probably arrive very close to the truth; at any rate we shall be able to judge whether they may be treated as one or the other without serious error: in this way we find that glass may be considered as elastic, and lead as a liquid.

Let A and B be two balls (for the sake of simplicity we will first suppose them equal and of the same material), and let A impinge on B with a velocity u.

Then at the instant of impact, before the motion of the centres of the balls has been affected, the only result will be the indentation of the surfaces at C, the point of contact; and it is clear that the surface of contact will be halfway between A and B, and

therefore C will approach A with a velocity $\frac{u}{2}$—and in the same way, that the relative velocity of B and C will be $\frac{u}{2}$.

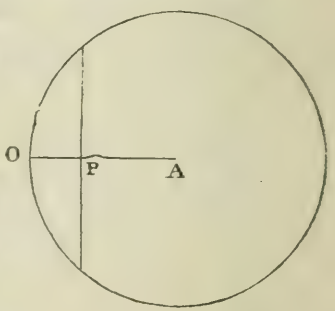
Hence in this case the first effect of the impact will be to impress a velocity on the surface of each ball relative to its centre equal to half the velocity of impact. If the bodies had been of different materials, the results would have been somewhat different, although of the same character; that is to say, these relative velocities would not be equal. In such case these velocities may be found in a manner which will be presently explained.

Now, in order to impress this velocity on the surfaces, pressure must exist between the bodies. This pressure will depend on the velocities and on the natures of the bodies. To show this, let

v = surface-velocity at C towards A, in feet per second.
λ = the modulus of elasticity of A,
d = the weight of 1 cubic foot,
p = the intensity of pressure in pounds on the square foot.

Then, for an elastic body, I shall show that

$$p=v\sqrt{\frac{\lambda d}{g}}.$$



Let O be the initial position of C, and x the initial distance of any point P in O A from O, and $x+\xi$ its distance at the time t.

Then for the equilibrium of a perpendicular lamina through P we have

$$\frac{d}{g}\times\frac{d^2\xi}{dt^2}=-\frac{dp}{dx}, \quad \ldots\ldots\ldots \quad (1)$$

$$p=-\lambda\frac{d\xi}{dx}; \quad \ldots\ldots\ldots \quad (2)$$

and from (1) and (2) we have

$$\frac{d^2\xi}{dt^2}=\frac{g\lambda}{d}\cdot\frac{d^2\xi}{dx^2}, \quad \ldots\ldots\ldots \quad (3)$$

the solution of which is

$$\xi=f\left(\sqrt{\frac{g\lambda}{d}}\,t-x\right).$$

And at C, where $x=0$,

$$\xi=f\left(\sqrt{\frac{g\lambda}{d}}\,t\right),\quad \frac{d\xi}{dx}=-f'\left(\sqrt{\frac{g\lambda}{d}}\,t\right),\quad \frac{d\xi}{dt}=\sqrt{\frac{g\lambda}{d}}\cdot f'\left(\sqrt{\frac{\lambda}{d}}\,t\right),$$

and $\dfrac{d\xi}{dt}=v$;

$\therefore$ substituting in (2), we have

$$p=\lambda f'\left(\sqrt{\frac{g\lambda}{d}}\,t\right),$$

$$\therefore p=\sqrt{\frac{\lambda d}{g}}\cdot v. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

This, then, shows the relation between the pressure, velocity, and nature of the body.

If u is the velocity of impact and λ', d' are the elasticity and density for the other body, we have

$$p=\frac{\sqrt{\lambda' d'}}{\sqrt{g}}\cdot(u-v);$$

and if the bodies are of the same material, we shall have

$$v=\frac{u}{2}.$$

If, however, they are of different materials,

$$p=\sqrt{\frac{\lambda d}{g}}\,v=\sqrt{\frac{\lambda' d'}{g}}\,(u-v),$$

$$\therefore v=\frac{\sqrt{\lambda' a'}}{\sqrt{\lambda d}+\sqrt{\lambda' d'}}\,u, \quad . \quad . \quad . \quad . \quad . \quad (5)$$

from which v may be found when u is known.

We now come to the case of a liquid When a liquid is projected against a surface, the pressure must be such as to overcome the momentum of the liquid; and in the case of continuous motion, if the pressure were uniform over the surface in contact we should have

$$p=\frac{v^2 d}{2g}\cdot v^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

And in the case of impulsive motion it is clear that the pressure cannot be less than this.

Hence if a stream of fluid B strikes a solid A, we have at the first instant

$$\frac{d'}{2g}(u-v)^2=\sqrt{\frac{\lambda d}{g}}\cdot v, \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

from which we can find v when u is known.

In the case of glass striking glass, we may take

$$d=d'=150,$$
$$\lambda=144\times 8{,}000{,}000=1{,}152{,}000{,}000,$$
$$p=1{,}440{,}000;$$

$\therefore$ substituting in (4), we have

$$v=\frac{1440000\times 8}{\sqrt{150\times 1152000000}}$$
$$=27\tfrac{1}{2} \text{ (feet per second).}$$

This would be the velocity just sufficient to crush the surface; to produce an appreciable effect would require a somewhat higher velocity; so that this value agrees very well with the result of experience, which is that a blast caused by a pressure of 4 inches of water is amply sufficient to cut glass.

In the case of an ordinary solid body we have, by combining the two effects,

$$p=\frac{\sqrt{\lambda d}}{\sqrt{g}}v+\frac{v^2 d}{2g},$$

which gives the pressure in terms of the velocity impressed on the surface. It is important to notice that, so far as the strength of the body itself is concerned, the last term $\frac{v^2 d}{2g}$ produces no effect, as this part of the pressure is balanced by the inertia of the surface itself, and that the first term $\sqrt{\frac{\lambda d}{g}}v$ will only hold so long as it is less than the ultimate pressure which the material will stand. When it is greater than this, the ultimate pressure must be substituted for this term.

In the case of lead we have

$$d'=712,$$
$$\lambda'=720{,}000\times 144;$$

but this will only be true so long as the pressure is less than 4000×144, this being the maximum pressure the lead will stand;

$\therefore$ instead of $\sqrt{\frac{\lambda d}{g}}v$ we substitute 576000;

$$\therefore\ p=576000+\frac{712}{64}(u-v)^2.$$

And if we take, as before, the pressure sufficient to crush the glass,

$$p=1440000,$$

we have

$$(u-v)^2=\frac{864000}{11}=78545,$$

$$u-v=280,$$

or

$$u=250,$$

since

$$v=30 \text{ nearly.}$$

Hence with lead the velocity must be about eight times as great as with glass.

XLIII. *On the Temperature and Physical Constitution of the Sun.* (*Second Memoir.*) *By* F. Zöllner.

[Concluded from p. 304.]

§ 7.

If M. Faye has recently started the question why the masses of hydrogen perpetually hurled aloft, through eruptive protuberances, from the interior of the sun have during the last thirty years not perceptibly augmented the chromosphere*, the above-given numerical values will suffice to answer this question, *on the supposition of its premises*, in accordance with the observations. Apart, however, from this, even the hypotheses on which alone M. Faye's question can have any meaning seem to me to be in direct contradiction to the view he has so often and so strenuously maintained, of the preponderantly gaseous constitution of the body of the sun; for the augmentation of the atmospheric envelope of a heavenly body through eruptions of gas from its deeper-lying parts is inconceivable unless the space whence the gas issues is enclosed by an envelope impervious to the outer atmosphere. But if such an envelope does not exist, or if the then unknown processes of eruptive-protuberance development take place outside the envelope and therefore in the lower strata of the atmosphere itself, the total quantity of this atmosphere can neither be increased nor diminished; for the volumes of gas thrown up are only portions of the atmosphere

* Vide *Comptes Rendus*, vol. lxxv. p. 1669 (Dec. 16, 1872):—"I have often asked myself whence came the hydrogenated flames of the chromosphere, which seem to be produced by continual violent eruptions. If the hydrogen issues unceasingly from the interior, how is it that it has not increased during the thirty years that protuberances, and even traces of the chromosphere, have been observed in eclipses, and during the three years that the chromosphere has been followed day by day? If it is not expelled beyond the sphere of the sun's action, it must, notwithstanding its specific lightness and the total absence of all indications of descending currents, in some way reenter the body of the sun."

which have merely changed their places through disturbances of equilibrium—that is, have been removed from its lower to its higher regions.

If, on the contrary, the sun is an incandescent liquid mass surrounded by a dense covering of vapours and gases which permeate each other in accordance with Dalton's law, then a portion of the gases (hydrogen for example) will be *absorbed* by the incandescent liquid surface, according to the proportion of the pressure at the base of the atmosphere*. Did, for instance, the atmosphere of our earth consist of carbonic acid of some atmospheres' pressure, the superficial portions of the seas would consist of water impregnated with carbonic acid, out of which, on a local rise of temperature or diminution of pressure, a portion of the gas must escape in the form of air-bubbles at the place in question, in order to restore the disturbed equilibrium of the atmosphere. Could these currents of carbonic acid gas be made visible, they would probably, by the analogies in their form and frequency, remind us of the solar protuberances. How great, moreover, is the capability of a water-surface to form bubbles on the escape of gas previously absorbed, and on the bursting of the bubbles to give occasion to suddenly vanishing resistances, one may at any time convince one's self by opening a bottle of soda-water. The height to which the fine drops of water are thrown, under the influence of the force thereupon developed, gives at the same time a means of measuring the initial velocities with which they leave the surface of the water.

That even incandescent liquids, at their surface, possess in a high degree a capacity for forming bubbles under analogous but highly magnified conditions is made plain by direct observations, namely Spallanzani's at the mouth of a crater in Stromboli:—

"The glowing lava rose every two minutes to a height of about twenty feet, and then sank with velocity back again into the depth. Each time, on reaching its greatest height, its surface puffed out, *bubbles of several feet diameter* swelled upwards and at length *exploded with a loud report*, bursting into a hundred fragments, which flew into the air with frightful violence and were precipitated rattling on the mountain as a shower of stones and scoria. . . . . . Poulett Scrope, in the year 1819, observed these phenomena take place in a precisely similar manner"†.

Two years since, on the ground of these analogies and facts,

* That incandescent liquids, even under a slight pressure, can dissolve and absorb gases is shown by the observations of St.-Claire Deville on the solution of gases in melting glass (*Comptes Rendus*, vol. lvii. p. 965).

† Naumann, *Geognosie*, 2nd ed. vol. i. p. 116 *et seqq*.

I formulated my view of the cause of the eruptive protuberances, and their connexion with the spots and faculæ, as follows*:—

"*The eruptive protuberances are produced by differences between the pressure of a quantity of gas enclosed in or absorbed by the liquid, and the external pressure of the atmosphere increased by the cohesion and weight of the upper layers of the liquid.*

"Accordingly eruptive protuberances will most readily arise in those places where the pressure to be overcome is the least, most rarely or not at all where this pressure is the greatest. Every ascending current in the atmosphere diminishes the pressure at the place, just as every descending current augments it. Now, as in the environs of the spots there are strong ascending currents and in like manner the faculæ are occasioned by currents of the same kind, these places must be peculiarly favourable for the development of eruptive protuberances"†.

Respighi, in 1870, expressed these relations as general results of his observations, thus‡:—

"On the contour of the spots there ordinarily arise gaseous jets of extraordinary intensity and violence and of well-defined form."

"Usually the protuberances and eruptions are very frequent and much developed in the localities of the faculæ," &c.

Similar results were also arrived at by Father Secchi in the course of his numerous observations, which he sums up, in

* *Berichte d. K. Sächs. Ges.* Feb. 11, 1871.

† M. Tacchini thinks that only those protuberances should be regarded as eruptions, which have the form of a tree or a fan—that is, are narrower at their base than at their upper end. Without discussing more in detail the claims of such a view, it is clear that, for the explanation of those eruptions, the physical conditions must necessarily be assumed to exist on the sun's surface without which an eruption (the violent and sudden overcoming of a resistance by compressed gas) is not conceivable. Such a resistance can only be caused by a substance which is in a more coherent state of aggregation than the gaseous; the existence of eruptive protuberances requires therefore necessarily the existence of a liquid mass, out of which the gases escape on the lessening of the pressure of the atmosphere resting upon them, like the bubbles from water containing carbonic acid.

Hence, when M. Tacchini, from the circumstance that among the numerous protuberances only some 8 per cent. exhibit the eruptive character mentioned, believes himself justified in concluding that the general form of the protuberances is irreconcilable with the assumption of a solid or liquid constitution of the surface of the sun, it is clear from the remarks above made that the principle of majorities is logically inadmissible in the present case; for even if only one single protuberance out of the great number observed were recognized as an eruption, we should be compelled to attribute at least to the place in question of the sun's surface those properties by which alone the physical possibility of an eruption becomes conceivable.

‡ *Atti della Reale Accademia dei Lincei,* Dec. 4, 1870.

the abstract of a communication to the Paris Academy*, as follows:—

"1. The regions of the faculæ and spots are the richest in protuberances.

"2. There are two sorts of protuberances:—one faint and light, spread like the light cirri in our atmosphere; the other denser, more compact, sharper, having a fibrous structure and peculiar optical characters."

That the more lively eruptions, which with greater velocity penetrate through the lower and more vaporous layers of the atmosphere, may drag portions of it upward above the lower limit of the chromosphere and exhibit its constituents spectroscopically by bright lines, if the eruption be sufficiently vehement, is a phenomenon to be expected. Corresponding to this, Father Secchi finds in the spectrum of the striking and intense protuberances above mentioned numerous lines of incandescent metals. He designates them on this account briefly as of metallic character, and (*l. c.*) sets forth their connexion with the spots in the following definite terms:—

"I then observed carefully all the eruptions having this character, which, for shortness, I will name metallic; and I found that every time one of these eruptions was observed at the eastern margin of the sun, a spot was visible on the following day. The connexion is so real that, during the last months, I have been able to predict the appearance of a spot by the mere inspection of the quality of the spectrum and the eruption. The five rotations of which I give the summary have themselves furnished twenty-four instances."

Father Secchi (*l. c.* p. 253) holds himself justified, on the ground of this connexion *in space* between the two phenomena, to draw a conclusion respecting the *nature* of their *causal* connexion:—

"The conclusion from all we have just said is manifest. The spots are produced by the eruption, from the interior to the exterior, of masses of the metallic vapours I have indicated."

Why the converse of the conclusion here denominated "manifest" could not also satisfy the observations ("the eruptions are produced by the spots")—that is, why the spot cannot, as an occasioning cause *precede* the formation of the eruptive protuberances, I have vainly sought in Father Secchi's works for a reason at all solid; for the reason of the fact above noticed, that at the eastern edge of the sun the upper portions of the protuberances first become visible, and on the following day, when the rotation of the sun has proceeded further, the till then concealed spot first comes into view, lies in relations so simple that it can-

* *Comptes Rendus*, vol. lxxvi. pp. 250–257 (Feb. 3, 1863).

not be assumed that Father Secchi has here allowed himself to be misled to the inference of *post hoc, ergo propter hoc.* To enter upon a closer consideration of the cyclone theory of the sun-spots, recently set up by MM. Reye* and Faye†, I think I may at present decline, since, in the animated discussion of these views in the Paris Academy, Father Secchi and M. Tacchini have brought out the contradictions to simple facts of observation into which the theory falls. I take leave to address to its advocates, without here discussing its physical bearings, the question put by Tacchini:—"In the face of observations so clear, so independent of hypotheses or preconceived ideas, is it possible to accept a theory which makes cyclones the sole cause of sun-spots?"‡. I am moreover bound to thank Professor Reye for the correction (in p. 177 of his work) he has made of an error in my memoir "On the Law of Rotation of the Sun and the Great Planets" (p. 91), relative to the transformation of the sun-spots into streaks, after the analogy of the formation of streaks of cloud-like products observed on Jupiter and Saturn. From Carrington's Table given by me in that memoir it is immediately seen that the difference between the velocities of diurnal rotation of two points of the sun's surface, the heliographic latitudes of which differ 1°, amounts on the average not to $1^{\circ}\cdot 6$ (as incorrectly given by me), but to $1'\cdot 6$.

If at the conclusion of these considerations I once more recapitulate the view I have invariably, since the year 1865, advocated, of the nature of the sun-spots as scoria-like products of cooling resulting from radiation at the incandescent-liquid surface of the sun§, it is only for the purpose of showing how simply, if it be adopted, all those phenomena can be explained by which the opponents of this theory have hitherto seen themselves necessitated to continual modifications of their views.

As the briefest possible expression of the essential point of my theory, I select the representation given in 1869, in the *Vierteljahrschrift der Astronomischen Gesellschaft*‖, and reproduce it *verbatim*:—

"The masses of scoria possess, as products of cooling, a considerably lower temperature than the glowing liquid mass, surrounding them on all sides, of the bright surface of the sun. The differences of temperature hereby determined must generate

* 'Die Wirbelstürme, Tornado's und Wettersäulen in der Erd-Atmosphäre mit Berücksichtigung der Sturme in der Sonnen-Atmosphäre, dargestellt und wissenschaftlich erklärt von Dr. *Theodor Reye,* ord. Professor a. d. Universität Strassburg.' Hannover, 1872.

† *Comptes Rendus,* vol. lxxv. p. 1664.

‡ *Comptes Rendus,* vol. lxxvi. p. 827.

§ *Photometrische Untersuchungen* (Leipzig, 1865), p. 246 *sqq.*

‖ Jahrgang iv. Heft 3, pp. 165–179.

in the atmosphere of elastic fluid above them currents *analogous to the land- and sea-breezes on our earth* as, according to experience, established by difference of temperature between land and water on the coasts of the islands. Therefore, along the coast of a scoria island on the surface of the sun, winds must be evolved, which in general are directed perpendicular to the coast; but these currents will, in the lower parts of the atmosphere, be directed outward from the interior of the island—in the upper, *i. e.* in the parts turned towards and visible to us, from without to the interior. There must thus arise at the borders of a mass of scoria whirlwinds, the axis of rotation of which follows horizontally its contour and is determined by it.

"In the parts of the atmosphere above the scoria, on account of the less radiation, products of condensation must of necessity be formed, which, cloudlike in their nature, will have their consistence and shape determined in great measure by the atmospheric currents which are directed to the centre. If now the vapours have attained the elasticity corresponding to the lower temperature over the scoria (which will obviously be more and more nearly the case as the moving portions of the atmosphere approach the centre of the island), the cause of cloudiness ceases, and we behold through the lightened and torn veil of cloud the underlying island of scoria as the nucleus of a spot.

"The boundaries of this island are therefore, according to the theory, still concealed by the penumbræ—which are the cloudlike products of cooling in the sun's atmosphere above a scoria-mass, becoming visible to us. According to this, the portions of the atmosphere over the nucleus of a spot must be regarded as filled with vapours the elasticity of which corresponds to the lower temperature over the scoria island. Hereby, taking also into consideration the experiment described in the last Part*, the widening of the dark lines of the spectrum where they inter-

* This is the experiment described by Kirchhoff in the year 1860 (Pogg. *Ann.* vol. cix. p. 297), by which it is shown that the insignificant quantity of incandescent sodium vapour in the mantle of an alcohol-flame can, *by virtue of its low temperature,* widen and darken the sodium-lines in the solar spectrum when the solar rays are caused to pass through those flames. On this Kirchhoff remarks, "There is at the first instant something surprising in the fact that the sodium in the little flame can perceptibly reinforce the action exerted upon the rays of light by the sodium in the immense atmosphere of the sun. But the surprise vanishes when we consider that the brightness of the lines D in the solar spectrum is determined by the temperature of the sun's atmosphere, preeminently of its outer layers, and that the temperature of these is certainly very much higher than that of a gas-flame." We see from this that the widening and darkening of some metal-lines in the spectrum of the sun-spots must not be regarded as an evidence of the *greater quantity* of absorbing vapours in those parts, but, primarily, only of the existence of a *lower* temperature of the absorbing gas-strata.

sect a sun-spot is explained. Whether we look through the broken cloudy covering right to the scoria, or to a mass of cloud or mist resting *immediately* above it, which must consequently be cooler and lower than the penumbra-cloud, may at first, as unimportant for the elucidation of the phenomena, be left undecided*.

"From the nature of these whirlwinds it results, further, that towards the centre of a spot there is a *descending current* in the atmosphere—on the contrary, at the outer borders of the penumbra an *ascending* one. It hence follows that *the inner border of the penumbra must lie deeper than the outer; and therefore the entire structure of clouds which appears to us as a penumbra must descend conically, or in the shape of a funnel, in the direction of the centre of the nucleus.*

"The observed widening of the penumbra on the side next the margin of the sun is thus explained, in the known manner, as a phenomenon of the perspective. Just so the peculiar radial shading of the penumbra finds its explanation in the direction of the atmospheric currents.

"Are the above-mentioned whirlstorms powerful enough to carry up into higher regions the stratum to which is assigned the name of the chromosphere, these parts will appear to us as protuberances. Taking into account the direction of the whirling motion, it follows that the ascending currents must be at the outer border of the penumbra; and this circumstance explains both the connexion of the protuberances with the sun-spots and also the processes of stronger light observed in the vicinity of the penumbra."

As a supplement to these explanations, I take leave at present to add only the following.

According to the views here developed, the space occupied on the sun's disk by the penumbra represents the region of circulation of those currents which are produced by the difference of temperature between the cooler mass of scoria and the surrounding glowing-liquid surface, in the superincumbent atmosphere. The lower currents, directed centrifugally from the centre of the spot, must, as cooled masses, lower the temperature of the portions of the sun's surface bathed by them. Taking into account the oppositely flowing upper current, in the penumbra we therefore look through relatively hotter upon relatively cooler strata of gas; so that through the breaks in the upper condensation-

* It is self-evident that we must not imagine our view of the mass of scoria of the nucleus of a spot to be so undisturbed and direct as that we have of the lunar surface. With the vapour-laden atmosphere of the sun and the still very high temperature of cooled scoria, it may be regarded as indubitable that our view does not fall direct upon the solid mass of the nucleus, but on the vapours condensed above it.

clouds in the penumbra a somewhat darker background becomes visible than in the other parts of the solar surface. Hereby is explained the less brightness of the penumbra in general, as well as the reversal of the relative brightness of the spectral lines which has often been observed in it and at its boundaries. A more precise reason for this phenomenon is obtained from the above-cited experiment of Kirchhoff, and from the causes, fully discussed in the description of it, by which the relative brightness of the spectrum-lines is determined. At the same time, by this view of the constitution and the physical causes of the penumbra, the relatively great variability of its boundaries becomes explicable.

§ 8.

Let me here be permitted a few remarks on determination of temperature of incandescent bodies in general. According to Kirchhoff's theorem*, the ratio of the emissive power E to the absorptive power A is a function (the same for all substances) of the temperature t and the wave-length λ; so that, independently of the particular nature of the substance, we have the following relation:—

$$\frac{E}{A}=J_{\lambda t}.$$

If the substance is not transparent, so that $A=1$ can be put, then is

$$E=J_{\lambda t}.$$

In relation to this yet unknown function, Kirchhoff (*l. c.* p. 292) remarks:—"It is a highly important problem to discover this function. There are great difficulties in the way of its determination by experiment; nevertheless there appear grounds for the expectation that it will be thus ascertained, since it is indubitably of simple form, as all functions which do not depend on the properties of individual substances have hitherto been found to be. Only when this problem is solved will it be possible for the whole fruitfulness of the demonstrated theorem to appear."

In my inaugural dissertation†, which was published shortly before Kirchhoff's memoir, and an abstract of which appeared in the same volume of Poggendorff's *Annalen*‡, I have determined photometrically the light emitted by galvanically incandescent platinum wire, and at the same time measured, by observation of the resistance and the intensity of the current, the amount of

* Pogg. *Ann.* vol. cix. p. 291 *sqq.*

† *Photometrische Untersuchungen, insbesondere über die Lichtentwicklung glühender Platindrähte.* Basel, 1859.

‡ Vol. cix. pp. 244–275.

heat developed in the wire with each value of the light-emission. The rays sent out by the wires were divided into two groups by the employment of a red and a green glass; and the variations of intensity of each were determined separately. The red glass absorbed all the rays from the violet end of the spectrum to the line D; while the green glass transmitted these rays, and absorbed those from the red end to the same line. Accordingly, for the intensities employed, the two glasses were nearly complementary, although in different degrees homogeneous.

On account, however, of the great interest which, as remarked above, is now attached to the knowledge of the relation between the temperature and light-emission of an incandescent body, I thought it would be worth the trouble to examine whether a simple relation subsists between the amounts of light observed by me to be evolved by galvanically incandescent platinum wires and the quantities of heat developed in them.

I take leave here to communicate first the data of observation taken from Table III. in the memoir mentioned (Pogg. *Ann.* vol. cix. p. 267), referring to the paper itself for a more precise account of the experiments.

The fundamental units and their proportion in the quantities of light for red light (J_ρ) and green light (J_χ) are unknown. In the arrangement of the observations the intensity of the galvanic current was always so regulated that with the different wires the quantities of light emitted were the same; the heat developed differed according to the proportion of the different thicknesses of the wires.

Wire No. I. had a thickness of 0·178 millim.; the other, No. IV., a thickness of 0·1035 millim. The quantities of heat developed were measured by the product of the resistance into the square of the current-intensity.

No.	Light.		Heat.	
	J_ρ (Red).	J_χ (Green).	w_{I} (Wire I.).	w_{IV} (Wire IV.).
1.	301	78	4·494	5·000
2.	670	174	5·011	5·387
3.	1170	409	5·413	5·948
4.	2500	831	5·984	6·345
5.	4132	1611	6·502	6·947
6.	5868	2450	6·600	7·338
7.	7500	3290	6·768	7·469
8.	8830	4333	6·908	7·601

A glance at these values shows immediately that there is no proportionality between the light evolved and the heat developed

in the wires, but the light-emission increases at a much quicker rate than the heat producing it. I therefore put

$$J = a \cdot e^{cw},$$

and assumed, in a preliminary experiment, that for both groups of rays $a = 1$; so that, on this hypothesis,

$$\frac{\log J}{w} = \text{const.}$$

The following Table contains the values of this quotient for each wire and for each of the two classes of rays.

No.	Red light.		Green light.	
	$\frac{\log J_\rho}{w_I}$.	$\frac{\log J_\rho}{w_{IV}}$.	$\frac{\log J_\chi}{w_I}$.	$\frac{\log J_\chi}{w_{IV}}$.
1.	0·5717	0·5054	0·4210	0·3784
2.	0·5653	0·5249	0·4470	0·4158
3.	0·5669	0·5158	0·4815	0·4391
4.	0·5678	0·5356	0·4880	0·4603
5.	0·5562	0·5205	0·4933	0·4616
6.	0·5709	0·5135	0·5136	0·4619
7.	0·5725	0·5188	0·5197	0·4709
8.	0·5714	0·5192	0·5265	0·5784

From this we gather that, for the more homogeneous red rays, the constancy of the required ratio indeed takes place with sufficient exactness. That the value of the constant a agrees closely with the arbitrarily chosen fundamental unit of light, is obviously a purely accidental circumstance. But, without approaching here to a more precise determination of the constant of the hypothetic function, the following brief remarks may be made respecting the less homogeneous green rays.

Supposing that, for rays of the same wave-length, the ratio between the quantity of heat developed in a body and its emissive power is universally the one assumed,

$$J = a \cdot e^{cw},$$

it is clear that, for a mass of rays which is not homogeneous, but, like that transmitted through the green glass, is continually taking up into itself new rays of less wave-length as the temperature rises, c cannot be a constant quantity, but must much more increase with rising values of w, in correspondence with a greater increase of the total quantity of light emitted. Now such a continual increase is in fact exhibited in the above values of the ratio $\frac{\log J_\chi}{w}$ for green light.

If in the present observations we may neglect the slight variations observed by Pouillet in the specific heat of platinum at high temperatures, the observed variations of the quantities of heat developed are proportional to the variations of temperature of the wires. The relation between the emissive power of a body and its temperature, for homogeneous rays, could then be represented by an analogous function, and thereby the determination by experiment of Kirchhoff's function in the way indicated, at least in its dependence on the temperature, be facilitated.

For practical objects, however, of the determination of the temperature of opaque incandescent bodies in an optical way, even a theoretical knowledge of that function would not be necessary, it would be sufficient to determine for several kinds of rays, in as different parts of the spectrum as possible, the constant in question, by which the light-emission as an exponential function is connected with the temperature of the incandescent body. Since this function must be the same for all opaque black bodies, independent of their other qualities, only the photometric comparison of the part in question of the spectrum with the analogous place in the spectrum of another opaque body of known temperature would be requisite, in order to deduce from the observed ratio of intensity, with the aid of that function, the unknown temperature of the second body.

To give a clear idea of the course to be pursued, suppose the following arrangement of the apparatus. In front of the slit of a spectroscope two right-angled reflection-prisms are fixed with the reflecting faces directed to opposite sides, so that the rays from two sources of light, one on each side of the spectroscope, are spread out into two adjacent spectra. One of the sources is a galvanically incandescent plate of platinum, for which, in like manner as in the above-described experiments, the constant of the function of the light-emission is empirically determined for a defined class of rays. Some direct calorimetric determinations of the temperature with different degrees of incandescence could without difficulty be effected under the given conditions, for a first approximation, to form the basis of the method. If now the other half of the slit be illuminated by an opaque incandescent body of sufficient extent, so that the apparent surface fills up the entire opening of the slit, the photometrically determined ratio of intensities of the two spectra at the places in question will, with the aid of the function ascertained for the platinum plate, immediately determine the temperature of the second body. For the photometric variation of the quantities of light the polarization principle would be suitable. For this purpose the rays from each of the two sources to be compared could, before they enter the reflection-prisms, be caused

to pass through Nicol's prisms the sections of which are perpendicular to one another, so that the two spectra in the field of the spectroscope would consist of lights polarized perpendicularly one to the other. A Nicol's prism brought into the ocular of the spectroscope would then alter, according to the known law, the ratio of intensities of the two spectra proportionally to the square of the tangent of the angle of rotation. The method indicated, however, is applicable only on condition that equally great portions of the apparent surfaces of the bodies to be compared send their light into the spectroscope, and also that the rays from the two bodies do not suffer different degrees of absorption before they enter the slit. The first condition, as already remarked, will be satisfied by sufficient apparent magnitude of the body; the second, on the contrary, will in general only be realizable for terrestrial substances, because the light of the heavenly bodies only reaches our apparatus after the loss of the light absorbed by the atmosphere, and, with the exception of the sunlight, is radiated from objects of vanishingly little apparent magnitude.

To remove this limitation of the method, I may briefly remark as follows. Suppose that in the field of view of the above-mentioned apparatus there are two spectra close together, the light of which is emitted from bodies at different distances, and the rays from which on their way to the spectroscope undergo different degrees of absorption. So long as the latter is not elective (that is, effected by coloured transparent media), the intensity-ratio of homologous parts of the two spectra can only be altered, by the circumstances referred to, in an equal degree for all kinds of rays. If, therefore, in the apparatus the brighter spectrum be so far enfeebled that the brightness of any kind of rays whatever corresponds with the homologous brightness in the other spectrum, then all the other homologous parts must agree photometrically in the two spectra, provided that both bodies have the same temperature. If the temperature is different, this accordance does not take place, but, when the two spectra have been made photometrically equal for a definite kind of rays (*e. g.* those corresponding to the line D), the more refrangible rays of the spectrum belonging to the hotter body will prevail over the homologous ones of the cooler body. The intensity-ratios which here take place are temperature-functions which must result from Kirchhoff's function. But even without the knowledge of the latter, this method, applied to the stars, would permit us to determine, at least qualitatively, their temperature-ratios—that is, to decide which of two stars possesses the higher temperature. With my astrophotometer, before the ocular of which a small direct-vision prism was placed, I have

made several experiments of this sort, and convinced myself of the general practicability of the method*. It is besides known to all observers of star-spectra, without the employment of photometric methods, that with the white stars in general the more refrangible portions of the spectrum appear much more intense than with the yellow and red stars. The temperature of the white stars must, accordingly, be in general higher than that of the yellow and red ones. When into connexion with this is brought Father Secchi's remarkable observation, that the different types of spectra are not distributed proportionately among the stars, but in certain regions of the heavens the one or the other type prevails, the inference is suggested that there are corresponding differences in the stage of cooling of these provinces of our fixed-star system, and that, if the time of origin of their consolidated masses was the same, these differ in magnitude.

§ 9.

If the law of the emission of light found above as an approximation for incandescent platinum wires were, as to its form, universal, and agreed with Kirchhoff's function J for opaque black bodies, it would necessarily be admissible to apply it to heat-rays also.

That between the heat-radiation of a body and its temperature there is no proportionality, I have already made clear in a criticism on the method applied by Father Secchi to the determination of the temperature of the sun †.

M. Soret has recently ‡ proved, by interesting experiments, that in fact the heat-radiation of a body increases much faster than its temperature, and that consequently the hypothesis made by Father Secchi in his actinometric determination of the temperature of the sun, "the radiation of a body is proportional to its temperature" §, was inadmissible.

M. Soret put a zirconium plate into vivid incandescence by a oxyhydrogen blowpipe, and determined the heat-radiation by means of the actinometer which had served him for measuring the solar radiation. On the hypothesis of the law of proportionality applied by Father Secchi to the sun there resulted for the temperature of the incandescent zirconium disk the value of 45,990° C., while in reality its temperature cannot have amounted to more than 2500°.

M. Soret's words (*l. c.* p. 228) relative to Father Secchi's formula are the following:—

* Vierordt has already made proposals for the photometric comparison of star-spectra. Cf. *Astron. Nachr.* (1871) No. 1863, p. 237.

† *Berichte d. K. Sächs. Ges.* 1871 (Feb. 11), p. 50.

‡ *Archives de Genève,* vol. xliv. pp. 220–229 (1872).

§ *Le Soleil* (Paris, 1870), p. 265.

"By employing this formula, and starting from the value of $t-\theta$ which I had obtained at the top of Mont Blanc, the Rev. F. Secchi arrived at the number $T=5335000°$*.

"To control the accuracy of this reasoning, let us apply it to the determination of the temperature of zirconium heated at the oxyhydrogen-lamp. We shall have

$$0°{\cdot}25=\frac{T}{183960},$$

whence

$$T=45990°,$$

a figure absolutely inadmissible; for the temperature of a body heated in the oxyhydrogen-flame is at the utmost 2500°."

In this experiment, therefore, the temperature calculated theoretically, according to the law of proportionality, was about 20 times the actual temperature of the incandescent body. If we had similar experiments at other temperatures of the heated disk of zirconium, we should be in a position approximately to construct the curve according to which the heat-radiation of the incandescent body increases with its temperature. Considering that here not homogeneous, but mixed rays come into operation, the curve would, analogously to the relation found above for the less homogeneous rays of the incandescent platinum wire, continually ascend more rapidly with rising temperature than for homogeneous rays. Apart from the character of an exponential-function, we should in the present case at all events have a right to assume that the ratio of the temperature found by Father Secchi, from the law of proportionality, to the true temperature of the sun's surface is greater than what was found by M. Soret for the ratio of the so calculated temperature of the incandescent zirconium disk to its true temperature.

If, in accordance with the above values, we take this ratio at 20, it will follow that the mean temperature of the sun's *surface* must at all events be *below* 267000° C. Of course this does not exclude higher temperatures at greater depths in the body of the sun. According to a correction (already given†) to p. 105 of my previous memoir, at a depth of about $\frac{1}{40}$ of the radius beneath the surface of the sun a minimum temperature of about a million degrees would result. In conclusion of this memoir I take leave to repeat the general remark made *l. c.* on determinations of solar temperatures, "*that, from the great want of accuracy in the empiric data necessary for these calculations, the values to be preliminarily obtained can only be rough approximations, settling rather the order of the temperature-quantities in question than the quantities themselves.*"

* The temperature calculated by Father Secchi for the surface of the sun.
† *Natur der Cometen*, p. 490.

XLIV. *On the Vibrations of Approximately Simple Systems.* *By* Lord Rayleigh, *F.R.S.**

THE meaning of the words "approximately simple" in the above title will be most easily explained by an example. Suppose that the "system" is a perfectly flexible string stretched between two fixed points. If the longitudinal density is uniform, the problem of determining the vibrations can be thoroughly solved. We know that in each of the fundamental modes the string vibrates as a curve of sines, and that the periods form a harmonical progression. These fundamental modes of vibration may coexist; and we know how to determine their amplitudes and phases so as to suit arbitrary initial circumstances. But when the density of the string varies from point to point, the solution of the problem cannot in general be effected; for it depends on a differential equation which has hitherto proved intractable. The question arises whether, when the string is nearly uniform, an approximate solution cannot be obtained which shall be correct so far as the first power of the deviation from uniformity. In this case the system may be called approximately simple, in the sense that a small alteration would make it simple, and bring it within the domain of exact analysis. The object of the present paper is to show that such cases admit of a perfectly general treatment by means of Lagrange's method of generalized coordinates.

Since the vibrations are supposed to occur in the neighbourhood of a configuration of stable equilibrium, and to be infinitely small, the potential and kinetic energies are expressed by homogeneous quadratic functions of the coordinates and velocities. By a suitable choice of coordinates (Thomson and Tait's 'Natural Philosophy, § 337) the terms involving the products of the coordinates and velocities may be got rid of, and the energies expressed as a sum of squares, with each term positive. Thus if ϕ_1, ϕ_2, &c. be the *normal* coordinates,

$$\left.\begin{aligned} T&=\tfrac{1}{2}[1]\dot{\phi}_1^2+\tfrac{1}{2}[2]\dot{\phi}_2^2+\ldots,\\ V&=\tfrac{1}{2}\{1\}\phi_1^2+\tfrac{1}{2}\{2\}\phi_2^2+\ldots,\end{aligned}\right\} \quad . \quad . \quad (1)$$

Now suppose that the system is slightly varied. The energies become

$$T+\delta T=\tfrac{1}{2}\big([1]+\delta[1]\big)\dot{\phi}_1^2+\ldots+\delta[12]\dot{\phi}_1\dot{\phi}_2+\ldots,$$

$$V+\delta V=\tfrac{1}{2}\big(\{1\}+\delta\{1\}\big)\phi_1^2+\ldots+\delta\{12\}\phi_1\phi_2+\ldots,$$

where, if new coordinates appear, their coefficients are small

* Communicated by the Author.

quantities; whence we obtain as the Lagrangian equations of motion:—

$$\big([1]D^2+\delta[1]D^2+\{1\}+\delta\{1\}\big)\phi_1+\big(\delta[12]D^2+\delta\{12\}\big)\phi_2 + \ldots=0,$$

$$\big(\delta[12]D^2+\delta\{12\}\big)\phi_1+\big([2]D^2+\delta[2]D^2+\{2\}+\delta\{2\}\big)\phi_2 + \ldots=0.$$

. . . .

In the original system the fundamental types of vibration are those which correspond to the variation of but one coordinate at a time. Let us fix our attention on one of them, involving, say, a variation of ϕ_r, while all the remaining coordinates vanish. The change in the system will in general carry with it an alteration in the fundamental or normal types; but under the circumstances contemplated the alteration is *small*. The new normal type is expressed by the simultaneous and synchronous variation of the other coordinates in addition to ϕ_r; but the ratio of any other coordinate ϕ_s to ϕ_r is small. The determination of these ratios constitutes the solution of the question proposed.

Since the whole motion is simple harmonic, we may suppose that each coordinate varies as cos pt, and substitute in the differential equations $-p^2$ for D^2. In the sth equation ϕ_s occurs with the finite coefficient,

$$-[s]p_r^2-\delta[s]p_r^2+\{s\}+\delta\{s\}.$$

The coefficient of ϕ_r is $-\delta[rs]p_r^2+\delta\{rs\}$. The other terms are to be neglected, inasmuch as *both* the coordinate and its coefficient are small quantities of the first order. Hence

$$\phi_s:\phi_r=\frac{\delta[rs]p_r^2-\{\delta rs\}}{\{s\}-p_r^2[s]}. \quad \ldots \ldots \quad (2)$$

Now approximately

$$-[s]p_s^2+\{s\}=0;$$

and therefore

$$\phi_s:\phi_r=\frac{\delta[rs]\,.\,p_r^2-\delta\{rs\}}{[s](p_s^2-p_r^2)},$$

the required result.

If only the kinetic energy undergo variation,

$$\phi_s:\phi_r=\frac{p_r^2}{p_s^2-p_r^2}\,\frac{\delta[rs]}{[s]}. \quad \ldots \ldots \quad (3)$$

From the rth equation we see that $-p_r^2[r]-p_r^2\delta[r]+\{r\}+\delta\{r\}$ is a small quantity of the *second* order; so that

$$p_r^2 = \frac{\{r\} + \delta\{r\}}{[r] + \delta[r]} = \mathrm{P}_r^2 \left\{ 1 - \frac{\delta[r]}{[r]} + \frac{\delta\{r\}}{\{r\}} \right\}, \quad . \quad . \quad . \quad (4)$$

where P_r denotes the value of p_r before the change. The interpretation is that the altered period may be calculated from the potential and kinetic energies without allowance for the variation of type which will in general accompany the change in the system, provided we are content to neglect the square of the change. This result, proved in a different manner, was given in a paper read before the Mathematical Society in June last*, and appears to be of considerable importance. It may be applied, for example, to the calculation of the beats of a slightly unsymmetrical circular plate, which are analogous to those often given by bells.

An example or two will probably clear up any point that may be obscure (whether from defective exposition, or the extreme generality of the method) in the general theory.

Consider the case of a string whose longitudinal density ρ is not quite constant. Since there is no change in the potential energy of a given configuration, $\delta\{r\} = 0$. On the other hand, if

$$y = \phi_1 \sin\frac{\pi x}{l} + \phi_2 \sin\frac{2\pi x}{l} + \ldots$$

be the form of the string at any time,

$$\mathrm{T} = \tfrac{1}{2}\int \rho \left(\dot{\phi}_1 \sin\frac{\pi x}{l} + \dot{\phi}_2 \sin\frac{2\pi x}{l} + \ldots \right)^2 dx = \tfrac{1}{2}\dot{\phi}_1^2 \int \rho \sin^2\frac{\pi x}{l} dx + \ldots$$
$$+ \dot{\phi}_1 \dot{\phi}_2 \int \rho \sin\frac{\pi x}{l} \sin\frac{2\pi x}{l}\, dx + \ldots .$$

If ρ were constant the products would disappear, since ϕ_1 &c. are the normal coordinates for a uniform string. As it is, the integral coefficients, though not evanescent, are small quantities.

Let $\rho = \rho_0 + \delta\rho$; then, in our previous notation,

$$[r] = \frac{l}{2}\rho_0, \quad \delta[r] = \int_0^l \delta\rho \sin^2\frac{r\pi x}{l}\, dx;$$

and therefore, by (4),

$$p_r^2 = \mathrm{P}_r^2 \left\{ 1 - \frac{2}{l} \int_0^l \frac{\delta\rho}{\rho_0} \sin^2\frac{r\pi x}{l}\, dx \right\}, \quad . \quad . \quad . \quad (5)$$

a formula from which the pitch of an approximately uniform string may be calculated. It should be noticed that no particular selection of ρ_0 is necessary, since P_r^2 contains ρ_0 as a divisor.

* "On some General Theorems relating to Vibrations," by the Hon. J. W. Strutt.

In order to find the variation of type, we have

$$\phi_s : \phi_r = \frac{p_r^2}{p_s^2 - p_r^2} \int_0^l \rho \sin \frac{r\pi x}{l} \sin \frac{s\pi x}{l}\, dx \div \frac{l\rho_0}{2};$$

or, since $p_r^2 : p_s^2 = r^2 : s^2$,

$$\phi_s : \phi_r = \frac{r^2}{s^2 - r^2} \frac{2}{l} \int_0^l \frac{\rho}{\rho_0} \sin \frac{r\pi x}{l} \sin \frac{s\pi x}{l}\, dx. \quad . \quad . \quad (6)$$

The normal component vibration is accordingly given by

$$y = \left(\phi_1 \sin \frac{\pi x}{l} + \phi_2 \sin \frac{2\pi x}{l} + \dots\right) \cos (pt - \epsilon),$$

where p has the value given by (5), and $\phi_s : \phi_r$ the ratio just determined.

As an application of this theory let us calculate the displacement of a node—for example, the node of the second component, which would be in the middle of the string, were it not for the want of uniformity. In the neighbourhood of $x = \frac{l}{2}$, the approximate value of y is

$$y = \phi_1 \sin \frac{\pi}{2} + \phi_2 \sin \frac{2\pi}{2} + \phi_3 \sin \frac{3\pi}{2} + \dots$$
$$+ \delta x \left\{ \frac{\pi}{l} \phi_1 \cos \frac{\pi}{2} + \frac{2\pi}{l} \phi_2 \cos \frac{2\pi}{2} + \dots \right\},$$

or

$$y = \phi_1 - \phi_3 + \phi_5 - \dots + \frac{\pi}{l} \delta x \{-2\phi_2 + 4\phi_4 - \dots\},$$

where

$$\delta x = x - \frac{l}{2}.$$

Hence, when $y = 0$,

$$\delta x = \frac{l}{2\pi\phi_2} \{\phi_1 - \phi_3 + \phi_5 - \dots\} \quad . \quad . \quad . \quad . \quad (7)$$

approximately, where

$$\phi_s : \phi_r = \frac{4}{s^2 - 4} \cdot \frac{2}{l} \int_0^l \frac{\rho}{\rho_0} \sin \frac{2\pi x}{l} \sin \frac{s\pi x}{l}\, dx.$$

The formula (7) gives the displacement of the node to the first order of approximation.

The generality of the method will perhaps be better brought out by an application to a bar of nearly uniform density, where the normal functions are more complicated. If these be denoted by u_1, u_2, &c.,

$$y=\cos pt\left\{\phi_1 u_1+\phi_2 u_2+\ldots\right\},$$

$$\mathrm{T}=\tfrac{1}{2}\int\rho(\dot{\phi}_1 u_1+\dot{\phi}_2 u_2+\ldots)^2 dx,$$

$$[r]=\int\rho_0 u_r^2 dx,\quad \delta[r]=\int\delta\rho\,.\,u_r^2 dx,$$

$$p_r^2:\mathrm{P}_r^2=1-\frac{\int\delta\rho\,.\,u_r^2 dx}{\int\rho_0 u_r^2 dx},$$

$$\phi_s:\phi_r=\frac{p_r^2}{p_s^2-p_r^2}\frac{\int\rho u_s u_r dx}{\int\rho_0 u_s^2 dx}.$$

I do not stop to discuss the physical bearing of these results; enough has probably been said to enable the reader to judge of the scope of the problem here considered, and of the simplicity of the mathematical machinery by which the solution may be obtained.

September 23, 1873.

XLV. *On the Determination of the Specific Heat of Gases and Vapours at Constant Volumes.* By R. C. Nichols.

[Continued from p. 290.]

THE uncertainty of the ordinary method is sufficiently indicated by the diverse results for the value of $\frac{c}{c_1}$ obtained by different observers, namely 1·348, 1·375*, 1·419. The last, however, that of Masson, does not greatly differ from the value calculated by the independent method from the velocity of sound, 1·4122.

The value of $c-c_1$ may be expressed by the general equation

$$c-c_1=\frac{\alpha}{\mu\mathrm{H}},$$

where α is the coefficient of expansion, μ the weight of a cubic foot of the gas estimated in any given unit, at 32°, per unit of pressure on the square foot, and H the mechanical equivalent of heat. This expression will obtain equally if α, μ, and H are taken for degrees Centigrade and metres.

If the ratio $\frac{c}{c_1}$ be represented by r,

$$c\left(1-\frac{1}{r}\right)=\frac{\alpha}{\mu\mathrm{H}},$$

which expresses a definite relation between the constants c, r, α,

* Gay-Lussac.

μ, and H; so that if any four of these are given, the fifth may be deduced independently of any other determination.

According to Regnault's observations, the density of air at 0° C. and a pressure of 760 millims. compared with mercury is 1 to 10517·3, by which estimation the value of μ for a cubic foot will be ·000038132. This gives $c-c_1=\cdot069166$, whence $c_1=\cdot16853$ if $c=\cdot2377$*. Assuming these values to be correct, $\frac{c}{c_1}=1\cdot4104$.

The accuracy of the result $c-c_1=\cdot069166$ depends on that of the values of the coefficient of expansion, ·003665 per degree Centigrade, the density of air as above, and the mechanical equivalent of heat, 772 for one pound of water raised 1° Fahr. If in addition $\frac{c}{c_1}$ be taken at 1·4122, $c_1=\cdot1678$, and $c=\cdot23696$, an independent determination of the specific heat of air for constant pressure, which is a little lower than that of Regnault.

The value of $\frac{c}{c_1}$, as calculated from the velocity of sound, has been given by Regnault at 1·3945; but this does not accord so well with his determination of the other constants as the value usually assigned, 1·4122.

The specific gravity of hydrogen being about ·0692, the value of $c-c_1$ for hydrogen is very nearly 1; and if E represent the atomic weight of any other gas (hydrogen being 1),

$$c-c_1=\frac{1}{\mathrm{E}}\text{ (nearly)},$$

a coincidence which is remarkable, though apparently accidental.

The observations of Regnault lead to the conclusion that the specific heat c of all simple gases at temperatures and pressures sufficiently remote from their points of condensation, varies inversely as their atomic weight; and since $c-c_1$ varies in the same manner, it follows that $\frac{c}{c_1}$ must have the same value for all these gases. This same conclusion was arrived at by Dulong from experiments on the velocity of sound.

Although the expression for the value of $c-c_1$ is perfectly general, it must be observed that it varies directly with the coefficient of expansion, α, which cannot be assumed to be the same for vapours or gases near their points of condensation as for per-

* The same result will be obtained from the equations given in the preceding part of this paper if the weight of 1 cubic foot of air be taken as 535·81 grs. at a pressure of 14·7346 lbs. and at 60°, instead of 535·68 grs. at a pressure of 14·706 lbs.

fectly elastic gases. Hence it is probable that $c-c_1$ is greater and c_1 less for aqueous vapour than would result from the general value of α.

Athenæum Club,
October 4, 1873.

Erratum in No. 306.

Page 289, last line of text, for $p(V_1-V_1)$ read $p(V_1-V)$.

XLVI. *On some Results of the Earth's Contraction from Cooling, including a discussion of the Origin of Mountains.* By James D. Dana.

[Continued from p. 289.]

V. *Formation of the Continental Plateaux and Oceanic Depressions.*

In my papers of 1846 and 1847 I attributed the formation of the great plateaux called continents, and of the oceanic depressions, to unequal contraction, observing that the oceanic crust, by being the later in consolidation, became more depressed through the continued contraction than the already firm or less-contracting continental crust. The steps in the process I propose now to consider, as a means of further elucidating the subject.

1. *Unequal contraction a fact.*

The *fact* of unequal contraction is manifest from the inequality of level that exists over the sphere, dividing it into oceanic depressions and continental plateaux; and unequal contraction (the material of the crust being essentially the same over the two kinds of regions) implies an unequal rate of cooling. Moreover it is a necessary conclusion that the great areas first consolidating should have been first free from that chemical activity and those ebullition-like movements due to escaping vapours which are inseparable from the fluid condition of rocks under merely atmospheric pressure.

2. *Location of the continental areas.*

The areas first to become quiet, and first to cool and consolidate, would be the shallowest areas—that is, those beneath which the solid nucleus of the globe reached nearest to the surface; for this approach to the surface would have been favoured by the chemical quiet, and the less depth would ensure more rapid cooling.

The solid state of the interior mass, under the Hopkins's theory, is due to the pressure of the outer portion, this pressure

being capable of producing an increase of density, and at some depth that density which belongs to the solid rock; so that downward either from the plane at this depth, or from some level or levels below it, actual solidity would have existed. It may be that when exterior solidification (that is, the solidification of the crust) was about to begin, the outer limit of the interior solid mass under the solidifying areas was quite up to the spherical plane in which the rock of the interior had the density of solidity. It is at least certain that its limit was as near the surface as was possible under the temperature then existing.

3. *Nature of the cooling crust, and of the liquid layer of which it was formed.*

The change of specific gravity or density which the rock-material underwent in passing from the liquid to the solid state is part of the data required for conclusions on the subject in view; and with reference to it we may first consider the nature of the rock-material.

The larger part of the igneous rocks of the globe are ejections, according to the evidence which has been presented, either from regions *within* the true crust of the earth (that is, the part situated below the supercrust and which was a direct result of the cooling), or from the plastic layer or seas underneath. In either case they are testimony, and have long been so regarded, with respect to the outer liquid layer of the melted sphere. The next best testimony we have is that from the earlier of the surface formations—the Archæan (Azoic). Another source of conclusions, appealed to effectually by Daubrée*, is the constitution of meteorites, and parallel facts in the earth's igneous and metamorphic rocks. Still another, used by Hunt, is reasoning from physical and chemical laws to the probable results under the supposed conditions of a cooling globe. We thus arrive at the following conclusions.

(*a*) The *more prominent minerals* were the following:—(1) some iron-bearing species (bearing also magnesia and lime) of the amphibole family, as *augite, hornblende*; (2) the iron-and-magnesia mineral *chrysolite* (or olivine), to which Daubrée, on the evidence just referred to, gives great prominence; (3) species of felspar—either the lime felspar called *anorthite,* the lime-and-soda kind called *Labradorite,* the soda-and-lime kind *oligoclase* (and andesine), the soda felspar (containing usually some potash) *albite,* the potash felspar (containing usually some soda) *ortho-*

* "Expériences synthétiques relatives aux Météorites. Rapprochements auxquels elles conduisent, tant pour la formation de ces corps planétaires que pour celle du globe terrestre," par M. Daubrée, *Comptes Rendus,* vol. lxii. (1866).

clase; of which felspars the first two are lowest in silica, 45–55 per cent., the third intermediate in amount of silica, or 60 to 64 per cent., the last two the highest, 65 to 75 per cent.; (4) *magnetite* (or magnetic iron-ore, $Fe^3 O^4$, but often titaniferous); and (5) perhaps *quartz* or free silica, and *mica*. Serpentine and chlorite are omitted from the list on the ground that they are always metamorphic. The question with regard to quartz is discussed below.

(*b*) The *principal rocks* in their relation to the subject before us fall naturally, as Elie de Beaumont first formally announced*, into two series, although there are intermediate kinds through which the two graduate into one another:—*one*, low in silica, or *basic*, containing less than 56 per cent. of silica; *the other*, high in silica, or *acidic*, containing 56 per cent. or more.

The *basic* rocks include dolerite and the related igneous rocks, made up of augite and Labradorite, with sometimes anorthite or oligoclase, often chrysolite, and generally magnetite in disseminated grains, and varying in specific gravity mostly between 2·8 and 3·2.

The *acidic* comprise (1) most trachyte and related felspathic rocks, consisting of one or more of the felspars oligoclase, albite, or orthoclase, with usually a little hornblende and magnetite, and sometimes mica, and not unfrequently free quartz—sp. gr. =2·5–2·75; (2) syenite, consisting of orthoclase and hornblende with quartz—sp. gr. =2·9–3·1; hyposyenite, consisting of orthoclase and hornblende without quartz—sp. gr. =2·9–3·2†; diorite, consisting chiefly of oligoclase or albite with hornblende—sp. gr. =2·80–3·1; granite, consisting of orthoclase (sometimes along with albite or oligoclase), quartz, and mica—sp. gr. =2·6–2·75.

(*c*) These igneous rocks are also conveniently arranged with reference to their origin into an *iron-bearing* and a comparatively *iron-free* series.

The *former* include the rocks containing as essential consti-

* *Bull. Soc. Géol. de Paris*, (II.) vol. iv. p. 1253 (1847). De la Beche has the idea in chapter xviii., on Igneous Rocks, of his 'Geological Researches' (1834). Bunsen uses it in his memoir on the Volcanic Rocks of Iceland (Pogg. *Ann.* vol. lxxxiii. pp. 201 (1851); and also Durocher later, in his memoir on Comparative Petrology.

† The name *syenite* belongs by right of priority to the quartz-bearing rock, as it was described from the locality Syene in Egypt, where that kind occurs; and I therefore call the kind free from quartz *hyposyenite*. Syenite is a rock of the hornblendic series in all its geological relations, graduating often into hyposyenite in Archæan regions; and it is bad to make it, as some German lithologists do, a hornblendic variety of granite. It deserves to stand as a distinct species; and it naturally leads off the hornblendic or syenitic series of crystalline rocks, as granite does the nonhornblendic or granitic series.

tuents one or more of the iron-bearing minerals (augite, hornblende, and chrysolite, and often also magnetite), and are divided into two groups—the *doleritic*, containing pyroxene, and the *syenitic*, containing hornblende in place of pyroxene.

The *latter* comprise those mostly (seldom wholly) free from these iron-bearing minerals, as the *trachytic* and *granitic* kinds.

(*d*) The *presence of quartz* among the chief constituent minerals of the true crust is not certain. Of the above-mentioned rocks, the basic iron-bearing (or doleritic) kinds are far the most abundant among acknowledged igneous rocks; and this fact seems to indicate that *quartz or free silica was not abundant in the original liquid rock of the globe*. Its *absence*, which Mr. Hunt urges, is seemingly opposed by the fact that it is present in so many trachytes, as well as in syenite and granite and the related rocks. But Hunt is right in holding that in general granite and syenite (the quartz-bearing syenite) are undoubtedly metamorphic rocks where not vein-formations, as I know from the study of many examples of them in New England; and the veins are results of infiltration, through heated moisture, from the rocks adjoining some part of the opened fissures they fill. These rocks, although common, present therefore no positive testimony on the side of the presence of quartz. Mr. Hunt urges, in support of his opinion, the experiment of Rose, in which fused quartz on cooling had the low density and other characters of the form of silica called opal, and not those of quartz. But the evidence is inconclusive, since a laboratory experiment cannot inform us what would be the condition of silica on cooling from fusion, provided the process of solidification took some millions of years. But Rose's experiment does seem to settle the question as regards all quartz veins, since, if of igneous origin, their little width would have ensured comparatively rapid cooling; and it thus sustains the evidence in favour of the view that such veins, like most mineral veins, were filled through the aid of heated moisture. Professor Hunt's argument from the probable condition of the material of the liquid sphere when about to solidify at surface—or the fact that the lime and magnesia now in our limestones and waters, and the soda in our waters, must have been mainly in the condition of silicates, and that therefore the free silica would thus be in combination—is of great weight; and, considering the vast amount of limestone in the earth's formations, it favours the view derived from the prevailing doleritic character of igneous rocks, that silica was mostly, if not wholly, in combination, and that the chief felspars present were the lime-and-soda species, Labradorite and oligoclase. Granite and syenite (common rocks of Archæan terrains) are just the rocks that are

likely to have been formed over the earth's surface after the action on the crust of the foul atmospheric vapours that settled upon it as it began to solidify. If there were mainly doleritic material and other Labradorite mixtures in that crust, the result of the conflict would be a removal of part of the bases and the liberation of silica, making free quartz and quartz-bearing rocks.

Again, the general fact that the doleritic rocks, and even most trachytic, contain disseminated grains of *uncombined* oxide of iron in the form of magnetite ($Fe^3 O^4$), adds to the strength of the argument against the general diffusion of quartz (that is, free or uncombined silica), if not proving its absence.

(*e*) The *presence of a large proportion of iron* is a marked feature of most igneous rocks. This ferriferous quality is not characteristic solely of the doleritic and syenitic kinds; even the most purely felspathic trachytes usually contain some disseminated magnetite and hornblende; and from this extreme there is a shading-off in trachytic rocks toward dolerites, syenites, or hyposyenites. It should be here understood that augite and hornblende are essentially identical in chemical constitution, though differing in crystallization. Hornblende has often a slight excess of silica, making the oxygen ratio of the bases and silica frequently $1:2\frac{1}{4}$, as in the felspar oligoclase, instead of $1:2$, as in augite; and this may be a reason for its occurrence by preference in the trachytes, in which oligoclase and orthoclase predominate, and that of augite in the dolerites, in which Labradorite is the predominant felspar. In Labradorite this ratio is $1:1\frac{1}{2}$, and in Andesine $1:2$. Mixtures of Labradorite and oligoclase, which constitute the base of some doleritic rocks (melaphyres, in which the silica constitutes over 55 per cent.), would have $1:2$ for this oxygen ratio when the proportion of Labradorite to oligoclase was 1 to 2.

The basic iron-bearing feature of the first solidified crust is attested also by the nature of the lowest rocks of the super-crust—that is, by the Archæan formations overlying the true crust and directly or indirectly made from it. We find among the Archæan terrains the felspar Labradorite far more abundant than in any later metamorphic rocks, the rock hypersthenite common (which is much like dolerite in elemental constitution), others (ossipyte and a chrysolitic hypersthenite) which are closely related to peridotite or chrysolitic dolerite, other rocks that are almost solely chrysolite, another (diabase) which has the composition of a chloritic dolerite, other kinds (referred to hypersthenite and diabase) which approach melaphyre. And besides, there are diorite, consisting of hornblende and albite or oligoclase, and hyposyenite, consisting of hornblende and orthoclase.

These iron-bearing kinds of Archæan metamorphic rocks much exceed in amount the granitic kinds that are free from iron. Moreover in these Archæan formations, the granitic as well as the iron-bearing, there are immense beds of iron-ore, as seen in New York, Canada, Northern Michigan, Missouri, Sweden, and Norway, the thickness of single beds frequently exceeding a hundred feet; and thus, although the surface over which Archæan rocks are exposed is relatively small, its iron is in vast amount. In fact, unlike human history, the earth's iron age was its earliest. Now, since these great iron-ore beds are of sedimentary or marsh accumulation (for they occur interstratified with quartzite, chlorite, schist, syenitic schist, and other metamorphic rocks), the iron was gathered from the preexisting crust-rocks; they therefore prove that iron was a very common ingredient in the original fused material of the surface of the liquid globe.

(*f*) We hence have reason for the inference that the original fused material contained largely the ingredients of the iron-bearing rocks dolerite, peridotite, diorite, hyposyenite, besides trachyte and the related kinds, and, *perhaps*, in small proportions those of the quartziferous trachytes, if not of granite and syenite.

It is not certain from present knowledge whether the slow cooling must not have made hornblende throughout the crust-mass in place of the akin species augite. Yet the considerations mentioned on the preceding page suggest that augite may have characterized the basic portions of the crust, and hornblende the smaller acidic portions; and if so, the prevailing rock is strictly doleritic.

(*g*) In view of the large proportion of iron, the *mean specific gravity of the true crust* can hardly be less than 2·9, and probably it is as high as 3·0.

(*h*) The *method of distribution* of the basic (doleritic) and the less abundant acidic (or trachytic) kinds in the earth's outer viscid layer before solidification—whether in separate layers, the latter over the former, as Durocher urges, or whether in separate local streams or regions made by the boiling movements and the great oceanic-like currents in the liquid mass, through the principle of liquation on a large scale—need not be here considered. I merely add that observed facts seem to be best explained on the latter view, since the existence of the two layers is not proved by the study of the Archæan terrains. If it existed at the beginning of solidification and the trachytic layer was thick enough not to have been obliterated by cooling before the era of the more recent trachytic ejections over the great Pacific slope, the constituents of the iron-bearing or dole-

ritic rocks should exist but sparingly in the Archæan instead of being the prevailing kinds.

4. *Change of Density and Volume in Solidification.*

All the rocks above mentioned have a higher density in the solid state than when in fusion. According to Delesse*, granite decreases in density in passing from the stone to the glass condition† 9 to 11 per cent.; syenite, syenitic granite 8 to 9 per cent.; diorite 6 to 8 per cent.; dolerite and melaphyre 5 to 7 per cent.; basalt and trachyte 3 to 5 per cent. The difference in volume is thus large between a rock in the solid and glass states. As to the difference between the glass and liquid states, we have no precise observations, and only know that it is exceedingly small‡.

From the above facts, sustaining the nearly total absence of quartz-bearing rocks from the crust, and therefore of granite or syenite, and the other considerations presented, we may take 8 per cent. as the probable average change of density for the earth's crust between the stony and the liquid states; which is equivalent to a change of volume from 100 to 92 per cent. in passing from the liquid to the stone condition.

5. *Process of Solidification and Continent-making.*

The crust over the areas of solidification, after attaining a thickness that would enable it to overcome by its gravity the cohesion in the liquid rock beneath, would sink in masses and then be remelted by the heat beneath, and this remelting would cool somewhat the liquid layer. So this process of crusting and sinking with an overflow from either side, remelting, and cooling, would go forward until the masses could sink without much remelting, to bring up at the level where the density of the liquid layer was that of the solid rock, if this liquid layer had not become so stiffly viscid by the cooling as to offer too great resistance to their reaching quite to this level. The sinking rock-masses may have had their density somewhat increased by the pressure to which they were subjected on descending; but whatever density they acquired,

* *Bull. Soc. Géol. de France*, part 2, vol. iv. p. 1380 (1847). Delesse's results agree nearly with those of St.-Claire Deville. Bischof in 1841 found the volume of basalt in the vitreous and crystalline states as 1 to 0·9298, the same in the fluid and crystalline states as 1 to 0·8960, and for granite the corresponding ratios 1 to 0·8420 and 1·07481.

† It is to be here noted that the glass and stone conditions are distinct molecular states of the same substance—the former produced under rapid cooling, the latter under slow,—and that common glass will become stone if solidified under a prolonged cooling-process.

‡ I am informed that at the Lenox Glass Furnace in Berkshire, Mass., no contraction is noticed in the cooling of the glass.

this density would determine the limit to which (setting aside resistance from viscidity) they would sink. It may be that portions went down until they came into contact with the nucleal solid mass. As the crust sank, the liquid material adjoining would continue to flow over the solidifying area and add to the solidifying material.

Finally, a layer of crust-rock miles in thickness would have been made over the great continental areas. Throughout the other portions of the sphere, the surface, whether all liquid or in incipient solidification, would have the level of that of the continental areas. For the sake of the illustration, suppose them to have been all liquid and the continental crust twelve miles thick, and the oceanic areas to go through the same process of solidification as had been completed over the continental areas; when, finally, the material of the oceanic regions had solidified down to the same plane with that of the continental (that is, to the twelve-mile limit), the oceanic crust thus formed would have become depressed in the consolidation (on the above ratio of 8 per cent. less volume for the liquid than for the solid) 5000 feet, or, if the layer consolidated was thirty-six miles thick, 15,000 feet—that is, supposing the continental part to undergo no contraction during the time. As such contraction would have been in progress from the continued cooling, the above 5000 feet is not the actual depth the basin would under the supposed circumstances have acquired; and yet, since the change of volume in the cooling of solid rock is small, it is not very wide of the fact.

The case here supposed is partly hypothetical, because the condition over the oceanic areas when the solidified crust of the continental areas was completed may have been that of incipient solidification, so that some of the contraction had already taken place. But, apart from this, it represents correctly, as it appears to me, the steps in the process, and illustrates how it is that great depressed areas would be an inevitable result, and why they should have comparatively abrupt sides or a basin-like character. The present mean depth of the oceanic areas below the mean level of the continental plateaux is probably about 16,000 feet. The thickness of the layer of liquid rock required to make a depression of 16,000 feet by its consolidation would be about $38\frac{1}{4}$ miles. But as contraction has gone on through time over both continental and oceanic areas, this is the mean excess of depression for the oceanic area. What part of this excess existed when the oceanic depression was first made, there are no facts for satisfactorily deciding. If the coral-island subsidence was due in any considerable part to radial contraction beneath the Central-Pacific crust itself, it is probable that the excess has increased even in Cænozoic time.

I find no explanation in the present state of science wherefore most of the dry land of the globe should have been located about the north pole, and of the water about the south. Physicists say that it indicates greater attraction, and therefore a greater density, in the solid material beneath the southern ocean. But why the mineral ingredients should have been so gathered about the south pole as to give the crust there greater density is the unanswered query. It may be that magnetite is much more abundantly diffused through the antarctic crust than the arctic. This is only one of many possibilities, and it is at present without a satisfactory fact to stand upon, beyond the general truth that iron was universally present.

6. *Resulting Crystalline Texture of the Crust.*

The doleritic and trachytic rocks of the true crust, and whatever else exists in its constitution, cannot be present with just the *texture* we find in the rocks of existing dykes and volcanic mountains. For, as above stated, the crust has cooled with inconceivable slowness, far more extreme than that which has attended the formation of any of the coarsest granites and syenites. And since the coarseness of crystallizations is generally in proportion to slowness of cooling, the texture of the whole should have been after the character of the coarse Archæan syenite or hyposyenite and vein-granite, or else in much larger crystallizations. One or another of the cleavable felspars is, in all probability, everywhere present; and since the cooling, and therefore the crystallization, has been in progress through many millions of years and still goes on, the probability is that the felspar crystallizations and cleavage-planes of the first-formed crust-layer were lengthened downward for very long distances, if not indefinitely. If so, the existence of a cleavage-structure in the crust as courses of easiest fracture, such as I have appealed to elsewhere in explaining the origin of the courses in the earth's great feature-lines*, is not an unreasonable supposition.

7. *The Continents always the Continental Areas.*

The above-stated effects of contraction lead to the necessary conclusion that the oceanic and continental areas were defined when the earth's crust first began to form—if not also still earlier, during the progress of its nucleal solidification. It is hardly possible to conceive of any conditions of the contracting forces that should have allowed of the continents and oceans in after time changing places, or of oceans as deep nearly as existing oceans being made where are now the continental areas,—although it is a necessary incident to the system of things that the con-

* Silliman's American Journal (II.), vol. iii. p. 381 (1847).

tinental plateaux should have varied greatly in their outlines and outer limits, and perhaps thousands of feet in the depth of some portions of the overlying seas, and also that the oceans should have varied in the extent of their lands. The many characteristics of the continental borders—for example, the contrast between the landward and seaward slopes of the mountains and even of the plications constituting them, the positions of volcanoes and of regions of igneous eruptions and metamorphism, indeed all the features declare which side of each border-chain is the oceanic and which the continental, and protest against speculations that would reverse the order. The early defining, even in Archæan time, of the final features of North America, and the conformity to one system visibly marked out in every event through the whole history (in the positions of its outlines and the formations of its rocks, in the character of its oscillations and the courses of the mountains from time to time raised), sustain the statement that the American continent is a regular growth. The same facts also make it evident that the oceanic areas between which the continent lies have been chief among the regions of the earth's crust that have used the pent-up force in the contracting sphere to carry forward the continental developments.

If this was true of the North-American continent, the same in principle was law for all continents.

Conclusion.

I here close this reconsideration of the views brought out in my papers of 1847. Of the principles then presented, and briefly recapitulated in the opening pages of this memoir (No. 303, pp. 42–44, I have found reason to modify some points connected with Section 4 (on mountain-making) and part of Section 8 (or that on metamorphism) :—the former in consequence of some new considerations of my own and of two ideas, of fundamental value, which I owe to LeConte*; the other in view of Mallet's recent contribution to Vulcanology. I purposely avoided in my early papers any expression of opinion as to the nature of the earth's interior (having had Hopkins's argument of 1839–1842 in view); and hence there is nothing on this point in the statements then made that requires change†.

The views on mountain-making now sustained suppose the

* Introduced in Part I. (No. 303, p. 50) and Part II. (p. 217).

† The later paper of Professor Hopkins, read before the British Association, did not appear in this country until after my articles of 1847 were in print; and I have since then been deferring my adoption of the views now accepted from it until the idea of the earth's interior solidity should have additional affirmation from the physical and mathematical side. This it has recently had through the writings of Sir William Thomson and others.

existence, through a large part of geological time, of a thin crust, and of liquid rock beneath that crust so as to make its oscillation possible, and refers the chief oscillations, whether of elevation or of subsidence, to lateral pressure from the contraction of that crust; and this accords with my former view, and with that earlier presented by the clear-sighted French geologist, Prévost.

I hold also, as before, that the prevailing position of mountains on the *borders* of the continents, with the like location of volcanoes and of the greater earthquakes, is due to the fact that the oceanic areas were much the largest, and were the areas of greatest subsidence under the continued general contraction of the globe.

Beyond these points there are additions and modifications. In addition to admitting the nucleal solidity of the globe, and the present partial union of the crust to the nucleus, these include the recognition of the following principles:—

(1) That in mountain-making on the continental borders, the oceanic crust had the advantage, through its lower position, of *leverage*, or, more strictly speaking, of obliquely upward thrust, against the borders of the continents.

(2) That among mountain elevations there are those which, like the Alleghanies, are the result of one process of making, or *monogenetic*, and those that are a final result of two or more processes at different epochs, or are *polygenetic*.

(3) That there are two kinds of monogenetic ranges:—those that are geanticlinals, or *anticlinoria*, like the region of the Cincinnati uplift; and those that were the result of a slowly progressing geosynclinal, with consequently a very thick accumulation in the trough of sedimentary beds, ending in an epoch of displacements and solidification, and often of metamorphism of the sedimentary beds, as in the case of the Alleghanies and other *synclinoria*.

(4) That great mountain-chains are combinations of synclinoria and of anticlinorian elevations.

(5) The principle advocated by LeConte (restricted as indicated), that plication, shoving along fractures, and crushing are the true sources of the elevation that takes place *during the making* of the second of the two kinds just mentioned of monogenetic mountain-ranges or synclinoria.

(6) That on the oceanic side of the progressing geosynclinal referred to there has been generally, as the first effect of the thrust against the continental border, a progressing geanticlinal which usually disappeared in the later history of the region—gravity, and the yielding and plication in the region of the geosynclinal, favouring this disappearance.

(7) That the locus of the region of subsidence on a continental border was in general along side of a region of thickly stiffened unyielding continental crust, and that pressure against the stable area beyond was one source of the catastrophe of mountain-making.

(8) That each epoch of plication and mountain-making ended in annexing the region upturned, thickened and solidified, to the stiffer part of the continental crust, and that consequently the geosynclinal that was afterward in progress occupied a parallel region more or less outside of the former, either landward or seaward, and commonly the latter.

(9) The principle adopted from LeConte, that the bottom of a geosynclinal becomes weakened as subsidence and surface sedimentary accumulations go forward, through the access of heat from below or the rise of the isogeotherms (the change of level in a given isothermal plane having been seven miles in the Appalachian region), and that this in an important degree has made possible the catastrophe from which synclinoria have resulted.

(10) That while igneous eruptions and metamorphism have each attended the formation of synclinoria, still in cases where the plication was greatest the igneous eruptions have been least in amount or absent; and that the most extensive igneous eruptions have taken place on continental borders after the crust had become too much stiffened to bend freely before the lateral pressure.

(11) That in the upturning and plication attending mountain-making the heat from the transformation of the motion was sufficient (in connexion with other heat from a rise of the isogeotherms due to previous surface-accumulations) to cause metamorphism, and also the pasty fusion which obliterates all stratification and gives origin to granite, and which may fill cavities or fissures, and so make veins that have all the aspect of true igneous ejections; and, as a more extreme effect, it may produce, as Mallet says, the degree of fusion which belongs to plastic trachyte, and give rise to trachytic and other ejections through fissures or volcanic vents. But—

(12) That the chief source of igneous rock is the plastic layer situated beneath the true crust, or the local fire-seas derived from that layer.

The discussion has enlarged beyond its limits in my previous publications; and many additional facts and conclusions have been brought forward. The various conclusions go forth to be tested by the further developments of science.

[*Note.*—The principle that the bottom of a geosynclinal becomes weakened as subsidence and surface-accumulation go

forward, through the rise of the isogeotherms, and that "this in an important degree has made possible the catastrophe from which synclinoria have resulted," is attributed (p. 374) to Professor LeConte. I should have credited to Professor T. Sterry Hunt the idea of the weakening of the bottom of a geosynclinal in the manner stated. To this idea Professor LeConte added the view that, through such a weakening, lateral pressure from the earth's contraction (a force not appealed to in Professor Hunt's hypothesis) was enabled to produce the catastrophe referred to; and this is the important principle adopted from his memoir.—J.D.D.]

XLVII. *On the "Black Drop" in the Transit of Venus.* *By* T. K. Abbott, *M.A., Fellow of Trinity College, Dublin**.

THE recent discussion in the British Association respecting the "black drop" observed in the transit of Venus, induces me to send you an account of a simple experiment which furnishes a very striking illustration of that phenomenon, and may be considered as affording an experimental proof that the moment of formation of the "drop" is the moment of optical contact. A common penknife and a threepenny piece (the thinner the better), or any two even edges, provided one is curved, constitute a sufficient apparatus. Bring the two edges into contact and hold them between the eye and a tolerably bright light, such as the flame of a candle, or better a lamp-globe, or even a window. At the moment of actual contact the edges appear to be, not touching, but joined by a short ligament, precisely as in the case of the transit. It is necessary that the eye be so adjusted that the point of contact shall be the point of most distinct vision; otherwise the ligament will appear longer but less distinct, and crossed by one or more short dark lines. If a coin with a milled edge be used, or a torn edge of paper &c., an appearance like Baily's beads is presented. It is easy to arrange the objects in such a manner that they can be fixed at any required distance from each other short of actual contact; and it will be found that the ligament is not formed until contact (actual or optical) occurs. Indeed it is only necessary to bring the fore finger and thumb together and hold them up to the light in order to observe the phenomenon; but it is less easy in this case to determine when optical contact occurs. It is, however, easier to "fix" the eye on the fingers than on the dark edges first mentioned. In each instance it is curious to observe the apparent adhesion of the ligament. An excellent example of the phenomenon may be produced by touching the edge or back of the knife with the point of a pin, holding the

* Communicated by the Author.

latter so as to form a very acute angle with the blade. The "ligament" will then be observed to fall from the point perpendicularly to the apparent edge of the knife. If the point of the pin be now slipped down behind the knife, the acute angle will appear to be partly filled up; and on moving the pin so as gradually to increase the angle (pivoting the pin round its point which is held below the edge), it will seem as if connected with the blade by a glutinous substance.

XLVIII. *On the Motions of Camphor and of certain Liquids on the Surface of Water.* *By* Charles Tomlinson, *F.R.S.**

1. On a former occasion I contributed to this Magazine two papers on the Motions of Camphor and of certain Liquids on the Surface of Water[1]. These papers contain a large number of references to phenomena which during nearly two centuries have cropped up in the Transactions and Proceedings of various scientific societies at home and abroad, and in many of the foreign and domestic journals devoted to science. Having studied these varied phenomena during a number of years and published many papers thereon, it was with sincere pleasure that I recognized in the same field of research a fellow-labourer who, armed with a new theoretical instrument, succeeded in gathering in the abundant crop which had been so widely scattered during so long a period.

By means of the principle of the surface-tension of liquids, Professor G. Van der Mensbrugghe, of the University of Ghent, has not only succeeded in binding together a magnificent sheaf of facts, but in doing so has contributed to the bundle a number of full and ripe ears of his own growing[2].

I must also take this opportunity of thanking him for the very kind terms in which he refers to my labours[3].

* Communicated by the Author.

[1] Phil. Mag. for December 1869 and January 1870.

[2] I must also associate with Professor Van der Mensbrugghe's name that of Professor Carlo Marangoni, of the R. Liceo Dante of Florence. In 1865 he published a pamphlet, *Sull' espansione delle goccie d'un liquido galleggianti sulla superficie di altro liquido*, in which he adopts the principle of surface-tension in studying the conditions under which a drop of one liquid spreads upon the surface of another.

[3] "M. Tomlinson avait étudié depuis plus de dix ans le phénomène de l'extension des huiles et les mouvements de certains corps solides sur l'eau; aussi je n'hésite pas à voir en lui le physicien qui a le mieux préparé la vraie théorie de ces phénomènes, grâce aux soins scrupuleux avec lesquels il a décrit les faits, en même temps qu'au nombre et à la variété de ses expériences; je me plais à ajouter que la lecture de ses travaux a le plus contribués à me suggérer les idées développées dans mon premier mémoire."

2. The object of his second memoir[4] is (I.) to furnish an additional number of facts by observers of repute bearing on the subject in hand (for which the author is in great measure indebted to the Royal Society's Catalogue of Scientific Papers), (II.) to meet certain objections that have been advanced against the theory, and (III.) to supply a few more experimental proofs.

3. In noticing the more remote facts, which are for the most part stated by Professor Van der Mensbrugghe in a very brief form, I must on several occasions enlarge considerably, since my own labours, begun so long ago as 1838, have in some cases anticipated more recent results. Many of my experiments have been already given in the two papers referred to above (see note [1]), so that in the present communication I shall endeavour as far as possible to avoid repetition.

4. And first as to the additional facts, omitting mere guesses, such as that of San Martino[5], who, in 1793, attributed the motions of camphor to electrical action, we come to Sir H. Davy[6], who, in 1802, noticed that fragments of acetate of potash (then called acetite) move on the surface of water somewhat after the manner of camphor. The more irregular fragments rotate most quickly, whence it is concluded that the shifting of the centre of gravity had something to do with the phenomena, while the rectilinear movements were attributed to currents descending from the salient points of each fragment and so producing on it unequal pressures.

5. Carradori, in 1805, noticed[7] that when a large drop of the milky juice of a euphorbiaceous plant is placed on the surface of water in a large vessel, a portion of the drop spreads, while another portion descends in threads which accumulate at the bottom. If, then, the vessel be inclined so as to spill some of the water and so renew the surface, on returning the vessel to its first position the agitation will cause some of the milky filaments to rise to the surface, where they spread like the first portion.

6. These and a multitude of other phenomena, as noticed in my first Experimental Essay[8], were attributed by Carradori to a

[4] "Sur la Tension superficielle des Liquides considérée au point de vue de certains mouvements observés à leur surface," second mémoire, extrait du tome xxxvii. des *Mémoires Couronnés* &c., publiés par l'Acad. Roy. des Sci. de Belgique, 1873.

[5] *Nuovo Giorn. Enciclopedico d'Italia,* Marzo 1793.

[6] Journal of the Royal Institution, vol. i. p. 314.

[7] "Dell' attrazione di superficie, Mem. II.," *Mem. di Matem. et di Fisica della Soc. Ital. delle Scienze,* vol. xii. parte 2ª. Many of Carradori's results and some account of his contest with Prevost are given in the first of the two papers referred to in note [1].

[8] Experimental Essays, published in Weale's Series, 1863. Essay I. On the Motions of Camphor on Water.

"superficial attraction" (*attrazione di superficie*), which Dutrochet afterwards developed into the "epipolic force" (*force épipolique*).

7. In 1819 Barlocci[9] covered fragments of camphor with gold leaf, and found that they did not move on the surface of water. The idea was that by preventing the formation of vapour, Venturi's theory, which attributed the motions to the escape of vapour, would be supported. It is further stated that perfect cubes of camphor do not move on the surface of water, on account of their perfect symmetry. This, however, is a mistake which several observers have fallen into; and we suspect that the cutting and shaping and handling of the camphor so as to confer upon it a geometrical form made it dirty, and that this was really the cause of its want of motion. I have frequently placed beautifully formed oblate spheroids of camphor on the surface of water; and these have continued to move about during some hours: but these figures were absolutely clean; for they were formed by dissolving camphor in strong sulphuric acid, and depositing a drop of the solution from the end of a clean glass rod upon the surface of clean water in a catharized glass $3\frac{1}{2}$ inches in diameter. The acid dialyzes off and leaves the camphor in a compact well-shaped button. Two or three drops may be deposited on the same surface; and the resulting buttons will sport about for a long time without interfering with each other's motions.

8. In 1824 a writer in Thomson's 'Annals'[10] attributes the motions of camphor on water to the fact that the centre of gravity of each fragment and the centre of support are not in the same vertical. Another anonymous writer[11] rejects this idea, on the ground that, if true, every irregular floating body ought to present the same phenomena as camphor.

9. I may here remark that during the many years in which the motions of camphor puzzled even the best observers, the idea was not seldom started that the shifting of the centre of gravity had something to do with the phenomenon. In 1863 Mr. Trachsel[12] quotes an experiment by M. Gingembre (without reference), in which a cylinder of camphor, ballasted at one end with lead, was placed in water reaching halfway up the little pillar. After twelve hours' contact the camphor was cut half through at the level of the water, but not equally all round. This experiment (which really originated with Venturi[13]) is quoted to show "the unequal power of solution in certain directions of the crystalline mass," and "this being one of the pro-

[9] *Giorn. Accad. di Sci. Roma*, vol. ii. p. 226.

[10] Second Series, vol. viii. p. 75.

[11] Ibid.

[12] Chemical News, August 22, 1863.

[13] *Annales de Chimie*, tome xxi. p. 262.

perties of camphor pointed out by Mr. Lightfoot, and from which he derives his well-written explanation of the cause of these motions when he says, 'the water upon which it floats, being capable of diffusing this vapour more readily in certain directions of the crystalline axis, thereby removes sufficient vapour-pressure at these points for the opposite side to drive about (by recoil) the nicely suspended particle.'"

10. In my reply to Mr. Trachsel[14], I state that nearly two years before I had pointed out the more rapid solution of camphor on a broken than on a natural surface, and also that the vapour theory was three quarters of a century old. It was first started by Volta in 1787, adopted by Prevost in 1799 under the term *jet gazeux*, and is now being revived without any experimental basis being provided for it.

11. As to the disturbance of the centre of gravity being the cause of motion in a floating and partly soluble mass, we have only to secure a crystal of some soluble salt to a slice of cork by means of an india-rubber ring, so as to be a little lighter than water; and if this be set floating, the waste will be constantly disturbing the centre of gravity of the mass, so as to produce a rolling over and a slow progressive motion. But no explanation of this kind suffices to explain the gyrations of camphor, benzoic acid, citric acid, &c., which are so rapid under the most favourable conditions (namely, chemically clean glass and water, and a bright, warm, dry day) as to extinguish the form of the fragments and make them appear like a grey cloud on the water.

12. In 1825 the brothers Weber[15] examined the subject of the camphor motions, and showed that a downy feather smeared with oil at the two ends will rotate on the surface of water. But other observers had already pointed out that an indifferent substance, such as sulphur, glass, earth, sugar, paper, &c. (as indicated by Carradori in 1808[16]), smeared with a fixed oil will rotate on the surface of water. But the first observation of this fact is older still. Franklin, in his celebrated letter to Brownrigg[17], on the action of oil in stilling the waves, dated November 7, 1773, states that while visiting Smeaton near Leeds, he was about to show what he calls "the smoothing experiment" on a pond, when Mr. Jessop, one of Smeaton's pupils, spoke of an odd appearance on that same pond. "He was about to clean a little cup in which he kept oil, and he threw upon the water

[14] Chemical News, September 12, 1863.

[15] *Wellenlehre.* Leipzig, 1825.

[16] *Giornale di Fisica &c.* vol. i. Pavia, 1808.

[17] Memoirs of the Life and Writings of Benjamin Franklin, edited by his grandson, W. T. Franklin (Lond. 1819), vol. ii. p. 268. Also Phil. Trans. 1774.

some flies that had been drowned in the oil. These flies presently began to move, and turned round on the water very rapidly as if they were vigorously alive." The experiment was repeated before Dr. Franklin, who says, "To show that it was not any effect of life recovered by the flies, I imitated it by little bits of oiled chips and paper cut in the form of a comma of the size of a common fly, when the stream of repelling particles issuing from the point made the comma turn round the contrary way." The Doctor adds that "this is not a chamber experiment; for it cannot well be repeated in a bowl or dish of water on a table." The reason for this is want of chemical purity in the bowl or dish. The effect may be well shown in a clean glass 5 or 6 inches in diameter, by moulding small coracles of paper on the rounded end of a glass rod, pouring into one of these coracles a few drops of a volatile oil, and placing it on the surface of water. The paper-comma experiment is sometimes referred to as originating with the brothers Weber; but in truth their labours added nothing to the subject, nor does it appear that they intended to do more than glance at it. They do not settle any thing, but, on the contrary, declare that the varied phenomena connected with the subject still remain unexplained[18]. They are even inclined to fall back upon an electrical theory in order to explain the rapidity with which a drop of oil spreads on the surface of water.

13. In 1828 August[19] described an experiment by Wirth, in which a metal ball is suspended at a short distance from the surface of water sprinkled over with powdered sealing-wax. On closing the hands over the metal ball the fragments of sealing-wax are set in motion, an effect attributed by Wirth to the magnetism of the human body, but by August to currents of air due to differences in temperature. He supports this view by showing that the effect can be produced by the action of a heated cylinder[20].

14. In 1836 Challis[21] endeavoured to account for the spreading of a drop of oil on the surface of water by considering that the angle formed by the free surface of this drop and the com-

[18] They say:—"Die ganze Erscheinung ist noch gar nicht erklärt."

[19] Pogg. *Ann.* S. 2. vol. xiv. p. 429.

[20] This class of experiment originated with B. Prevost at the beginning of the present century (*Ann. de Chim.* vol. xxiv. p. 31). These experiments (of which the following is the leading one) are remarkable, and excited a good deal of attention at the time. If near the edge of a disk of tinfoil floating on the surface of water we present obliquely a rod of heated metal, or the focus of solar rays by means of a burning-glass, the disk moves away from the source of heat. This experiment, now so easily explained on the principle of surface-tension, led Prevost into some very elaborate speculations.

[21] Phil. Mag. vol. viii. p. 288.

mon surface of the two liquids must be very small, and that consequently a thin film of oil must spread over the whole surface of the water, while a second drop would assume the lenticular form. I give Professor Challis's own words, since a similar theory was published by Du Bois-Reymond[22] in 1858. He says:—"The angle of actual contact between two fluids is determined by the hydrostatical equilibrium resulting from the molecular attractions of the two substances, the fluids being treated as incompressible. It thence appeared that this is an exceedingly small angle in cases in which the bodies in contact are not of very different specific gravities. Hence in the instance before us, the angle of contact (that is, the angle which the surface of contact of the oil and water makes with the upper free surface of the oil) is very small. But since the drop is convex both at its upper and under surfaces, this is apparently an angle of considerable magnitude. In fact the theoretical angle of contact, or that which the upper surface of the oil makes with an imaginary surface drawn parallel to its under surface, and just beyond the sphere of the molecular action of the water, would be found by calculation to be of sensible magnitude. Consequently, that the angle of actual contact may be exceedingly small, the portion of the upper surface of the oil that lies within the sphere of the molecular action of the water must undergo a flexure near the visible periphery of the drop. Now in fulfilling this condition it seems probable that a very thin film of the oil spreads over the whole water surface (as there is no force to counteract), and gives rise at the same time to the visible spreading of the first drop. The film itself, being of less thickness than the radius of the sphere of the molecular action of the water, will not be perceptible to the senses. Such a circumstance having happened to the drop that first comes in contact with the water will prevent any that succeed from being similarly affected."

15. It is remarkable that Professor Challis should have thought it theoretically probable that a thin [invisible] film of the oil spreads over the whole surface of the water and assists the visible [and slower] spreading of the first drop, because in some oils (such as oil of cinnamon) this thin film is visible. On depositing a drop of this oil on the surface of clean water in a clean glass $3\frac{1}{2}$ inches in diameter, an exceedingly thin but visible film is instantly drawn over the whole surface of the water, while the denser film, which does not cover half the surface, spreads much more slowly. In the case of freshly distilled oil of coriander (the very remarkable cohesion-figures of which are given in one of my papers[23]), the first thin film is accompa-

[22] Pogg. *Ann.* vol. civ. p. 193.

[23] Phil. Mag. S. 4. vol. xxxiii. plate iv., June 1867.

nied by volleys of minute globules which extend to the edge of the water in radial lines. Other similar cases of essential oils might be cited; and even in the case of solid fats the evidence of a thin invisible film covering the surface and arresting the camphor motions, seemed to be irresistible before the promulgation of the surface-tension theory. A stick of common mottled soap, or of tallow or lard, lowered into a large surface of water previously dusted over lightly with lycopodium powder, instantly clears away a large circular space (extending nearly to the edge of the glass). This effect is much more satisfactorily explained on the surface-tension theory than on the existence of a thin invisible film. But in the case of oils, whether volatile or fixed, it must be considered that a much greater tensile force is exerted in spreading some oils than others. When a film formed by a drop of a volatile oil nearly covers the surface of the water, there is still a sufficient residual amount of surface-tension to cause fragments of camphor to perform their evolutions; they skate through the film, cutting it up in all directions. But in the case of a film formed by so large a quantity as a drop of any fixed oil, the camphor fragments are motionless, whether on the film itself or on the adjacent water-surface. This was formerly supposed to prove the existence of a fatty film which, though invisible, destroyed contact between the water and the camphor; but now, in accordance with the new theory, the fatty-oil film so far reduces the tension as to leave no residual force sufficient to give motion to the fragments.

16. But if, as I have shown, a freshly distilled volatile oil does not prevent the motion of the camphor fragments, some deductions must be made from the statements of the earlier observers, which savour of the marvellous. Thus Volta, writing in 1787, says:—"If the water be defiled with any foreign substance, or its surface only slightly fouled with oily matter, if only the dust of the room or of one's clothes be upon it, the looked-for motions of camphor and of benzoin will not take place, or will be so feeble as to be scarcely sensible[24]. So also Prevost[25], writing in 1797, remarks that "if the surface of the water be touched with a pin previously dipped in oil, the motion of the camphor fragments instantly ceases, just as if they had been struck by lightning (*comme foudroyées*)." Venturi[26] also, in 1797, says, "Touch the surface with oil, and an almost imperceptible film instantly advances over the whole surface, repels the fragments, and strikes them motionless as if by magic."

[24] Volta's Latin letter to Frank is contained in the *Delectus Opusculorum Medicorum*. Ticini, 1787. An English translation of this letter is given in my 'Experimental Essays.' See note [8].

[25] *Ann. de Chim.* vol. xxi. p. 254. [26] Ibid. p. 262.

17. Now in all these cases I venture to say that the experiment was not performed under the most favourable conditions. It was known that the water must be free from grease; but I know of no reference on the part of these early inquirers to show that they took special means to make the vessel containing the water chemically clean, any more than the surface of the camphor or the knife with which it was cut or scraped. Hence, the initial tension of the water not being at a maximum, contact with a very slight portion of grease or oily matter would so far further lower it as to produce the sudden effects described. But if the experiment be repeated under the most favourable conditions, in which the surface-tension of the water is at its maximum, the water may be touched with a fixed oil, and the iridescent film resulting therefrom, though lowering the tension, will not do so to a sufficient amount to arrest the motions of the camphor fragments. For example, a shallow glass 4 inches in diameter was filled first with strong sulphuric acid and then rinsed out with tap-water and filled with the same. Fragments of camphor were extremely active on the surface, which was now touched with the point of a clean penknife that had been dipped into refined East-India rape-oil; a film of a splendid deep-blue colour was produced, which instantly opened into a sort of lace-pattern; but the fragments of camphor continued to rotate, not so vigorously as at first, but still with vigour. If the film of oil be first formed on the surface and the camphor fragments thrown upon it, they will also rotate. If instead of using the point of a knife to deposit the oil, a glass rod be employed and a drop be deposited, this, especially if the oil be heated, will flash out into a film, completely covering the surface of water in a vessel 6 inches in diameter. Under these conditions fragments of camphor do not rotate; but there is sufficient residual tensile force to give motion to creosote. A drop of this instantly repels the oil-film, cuts it up in all directions, and moves over the surface with great vigour, the only sign of diminished tension being that the cohesion-figure does not open out into the usual brittle arc and so break up into a number of figures, but preserves one parent figure, from which volleys of small globules are discharged[27].

18. In the paper just referred to[28] I give a number of cases in which a volatile-oil film arrests the motions of creosote, and then evaporating, the water (according to the new theory) so far recovers its tension that the creosote suddenly starts into life and increases in vigour as evaporation proceeds.

[27] Figures representing this action are given in the plate accompanying the paper referred to in note [23].

[28] See note [23].

19. Considering the mode in which surface-tension acts, it will naturally be supposed that the motions in question are not confined to camphor and a few other solids, nor to creosote and a few other liquids. In fact fragments of any body that act by suddenly lowering the surface-tension at the spot on which they fall are liable to these camphor motions. Many salts act in this way, the last that I have observed being sulphate of aniline. The following solids also rotate:—Borneol, naphthol, thymol, nitrotoluol: this was inactive on water at 56° F., but very active on water at 90°, a crystalline needle sweeping over the surface at right angles to the length of the crystal. Binitrotoluol on water at 96°. Hydrochlorate of toluidin: a crystalline fragment darted to and fro in short jerks, as if surprised at the novelty of its situation, and then, accepting the conditions, moved rapidly over the surface in circular sweeps and quickly disappeared. Acetamid behaved in a similar manner. Lactid and oxamethan also rotate rapidly. Sulphaldehyd rotates rapidly in wide sweeps and soon becomes motionless. The same phenomena are exhibited by benzoic anhydride. Amidobenzoic acid also rotates rapidly, and quickly disappears; while the crystalline portion of nitrobenzoic ether sails round and round near the edge, and continues to do so for a long time.

Among liquids, a drop of toluidin on the surface of water, in a vessel 4 inches in diameter, affords an instructive illustration of surface-tension: the drop forms a figure for an instant something like that of creosote; it then bursts as with an explosion, dotting the whole surface with smaller figures, each of which also explodes; and the smaller particles resulting therefrom rapidly disappear amid lively agitations. A drop of phenyl-mustard oil quickly spreads into a large ragged film, which is torn into numerous fragments, covering the surface; and then each fragment gathers itself up into a well shaped lens. A second drop rests as a lens. A drop of monochloracetic ether spreads rapidly over the surface and rebounds to the centre, breaks up into numerous ovoid masses, with colour, which rapidly disappear with a remarkable opening out. When the whole has disappeared, the motes on the surface rush wildly to and fro. Formiate of ethyl produces a very pretty rose-engine figure with a central boss. Oxalate of ethyl forms a wide film with iridescent edges, and then, in waving figures, quickly disappears. Iodide of allyl gives a colourless film over the whole surface, then exhibits a magnificent display of iridescence, breaks up and instantly disappears. The following liquids also present interesting phenomena:—Nitrobenzol, nitrobenzoic ether, nitrotoluol, and cymol. It would be easy to extend the list both of solids and liquids that afford instructive illustrations of surface-tension,

whether on the surface of water or on that of other liquids. A drop of oil of lavender on the surface of liquid acetic acid forms a wonderfully active figure, which I have already noticed[29], together with the figures of various liquids on the surfaces ot sulphuric acid, cocoa-nut oil, castor-oil, paraffin, spermaceti, white wax, olive-oil, lard, and sulphur, heated, where necessary, so as to render them sufficiently fluid[30].

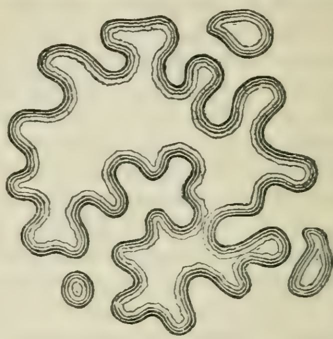
20. In 1837 Dr. Pietro Savi[31] had his attention directed to the following statements by De Candolle[32] with reference to the contractile motions of plants:—"Si l'on place sur l'eau des folioles, ou des fragmens de folioles, du *Schinus Molle,* on voit l'huile volatile, contenue dans certaines cellules du tissu, s'echapper, non par un flux continu, mais par des saccades intermittentes, qu'on ne peut, ce me semble, rapporter à d'autres causes qu'à quelque contraction des cellules qui renferment ce suc." And again at page 287 of the same volume, with reference to the sudden and singular movements of the leafy fragments:—"ces mouvemens sont dus à des jets intermittens d'huile essentielle, qui sortent des cellules, frappent l'eau, et determinent dans le foliole un mouvement de recul semblable à celui de l'éolipyle. On voit ici assez clairement un effet vital."

Dr. Savi shows, by a microscopical examination of the leaves in question, that no provision is made for the exertion of a contractile force, that fragments of the dried leaves move, that the motions are common to leaves of the *Terebintaceæ, Euphorbiaceæ, Urticaceæ, Asclepiadaceæ,* and some others, and that the true explanation of these phenomena (which are physical, not physiological) is to be found in Carradori's attraction of surface, as explained in his memoirs[33].

21. In 1838 Morren[34] showed that a leaf of the *Schinus Molle,* or American pear, placed on water had a jerking motion, while the surface became covered with a film of a sweet-smelling oil. Another plant, the *Passiflora fœtida,* is furnished with hairs, one of which, plunged into water, discharges a small drop of oil, which, rising to the surface, expands and contracts several times, and then apparently bursts with violence, the smaller portions going through similar changes. These are well-known effects of surface-tension common to several oils and other liquids, as is also an experiment by Zantedeschi[35], in which a

[29] Phil. Mag. for March 1862.
[30] Phil. Mag. for November 1864.
[31] *Mem. Valdarnesi,* 1837, vol. ii. p. 117.
[32] *Phys. Végét.* vol. i. p. 38.
[33] He refers to *Mem. della Soc. Ital.* vols. xi., xii., and xv.
[34] *Corresp. Mathém. et Phys. de* M. Quetelet, 1838, vol. x. p. 339.
[35] Given by Luvini in his *Saggio di un Corso di Fisica Elementare*

few drops of valerianic acid poured into water rise to the surface, go through a variety of contortions and evolutions, and excite the astonishment of the observer that dead matter should thus of itself become endowed with such active motion. Some years ago I performed this experiment, and had a drawing made of one stage of the contortions. If the annexed figure is supposed to be in rapid motion, constantly changing into simi-



lar figures, and detaching pear-shaped lenses which immediately become circular, a good idea will be formed of the behaviour of this liquid on the surface of water. A drop of isobutylic alcohol forms a similar figure, only more rapid in its action.

22. In 1838 I published a new method of producing Newton's rings[36]. Referring to the facility with which oil spreads over the surface of water, it is stated that any oil, balsam, varnish, or essence not soluble in water will answer the purpose, or, if the oil or balsam &c. be too viscid, it may be made sufficiently fluid by the action of heat. A single drop of oil &c. may form a circular film from 2 to 5 inches in diameter with or without colour, depending on the thickness. Supposing the film to be without colour, the finger dipped in ether (or a piece of porous wood, or a camel's-hair pencil, or, as was afterwards adopted, a bit of sponge tied over the end of a glass rod) and held over the film, "we instantly see a repulsion or recession of particles; a black spot is formed; and round it the seven orders of rings appear in all their lustre and beauty. The rings remain just so long as the finger is wet with ether, which having evaporated, or the wet finger being removed, the rings contract in diameter and disappear altogether. These rings can be formed at any part of the surface of the film, and any number of times while the film endures."

Among the conditions enumerated are:—"the glass and the water must be perfectly clean and free from grease; the surface of the water must be perfectly tranquil; a clean glass rod must be dipped into the oil, and a single drop only be placed

(Torino, 1868), p. 236. Professor Luvini has copied, at pp. 238, 239, the figures of creosote and coriander referred to in notes [23] and [27].

[36] The Student's Manual of Natural Philosophy, London, 1838. In the chapter entitled "The Soap-bubble," at p. 545.

upon the surface of the water; the ether must not touch the film, or it will destroy it, while if the ether be taken up by a body that has not a pointed or rounded termination the rings will not be formed, and there will only result a series of concentric figures, depending for form upon that of the end of the substance held over the film. Instead of ether we may employ solution of ammoniacal gas, pyroligneous ether, alcohol, or naphtha. Strong ammonia sometimes breaks up the film and scatters it over the surface of the water."

A turpentine film, which is at first colourless, soon begins to display colour as the film becomes thinner by evaporation—a remark that does not apply to a film formed by a drop of the fixed oils of olive, rape, castor, nut, &c.

Films that exhibit some of the most splendid colours of thin plates may be formed from a drop of balsam of Peru or a turpentine varnish, such as copal varnish, carriage-varnish, gold size, black japan, &c. If the ether be held over one of these coloured films, "we immediately get a series of rings as before, quite independent of the large rings which occupy the surface of the water. That part of the film subjected to the action of the ether no longer forms a portion of the original film, but is subject to the systematic arrangement which the ether produces. A film showing the colours of the 5th, 6th, and 7th orders only is, at that particular part which is subjected to the action of the ethereal vapour, made sufficiently thin to exhibit the rings of the 1st, 2nd, 3rd, and 4th orders, or of the 2nd and 3rd, or of the 3rd and 4th,—all depending on the quantity of ether taken up, the rate of evaporation, and the proximity of the ether to the film—in other words, on the comparative thinness of the film. Should the drop of oil &c. form a lens instead of a film, the ether vapour will drive it with great energy over the surface of the water."

If the water be dusted over with a dry powder, the vapours of ether &c. will powerfully repel the particles. "Turpentine and some of the volatile oils produce a similar effect upon powders; and one volatile oil frequently repels a film formed by another."

The above details refer to the repulsion of films. "A strong and decided attraction may be exhibited by presenting to the film a drop of an acid of low boiling-point, such as nitric or pyroligneous acid. By such means an oil-film of the size of a crown-piece may be reduced to the size of a sixpence; and a film exhibiting colour will have all colour destroyed by the proximity of the acid, whose influence is to thicken the film. Sulphuret of carbon and an aqueous solution of chlorine produce a similar result" (pp. 545–549).

23. In 1841 Sir David Brewster[37] reproduced some of my results, probably without suspecting that he had been anticipated, although a full abstract of them was given in a then popular Journal of wide circulation[38]. Brewster formed a film of oil of laurel on water placed in a black vessel, or on the surface of diluted or real ink. He justly describes the rings produced as being splendid beyond description. He says:—"These thin plates of oil of laurel exhibit some curious phenomena which I believe have not been noticed. If we wet with water, alcohol, or the oil of laurel itself, the extremity of a short piece of wire, such as a large pin, and hold the pin in the hand so that its head may be above and almost touching the film, the film will recede in little waves of circular shape which form a new system of coloured rings; and they become covered with the vapour from the fluid on the head of the pin in such small particles that they reflect no light, and the rings appear to be blackened. By withdrawing the pin the film is restored to its former state. The same effect is produced by heating the pin or the fluid upon it to promote evaporation" (p. 51, note).

24. We now come to the time when Dutrochet published his elaborate essays on what he termed the epipolic force; and as this forms a kind of middle term between the earlier and the more recent history of this wide subject, we may here conveniently pause for a time.

Highgate, N.
7th October, 1873.

XLIX. *On Integrating Differential Equations by Factors and Differentiation, with Applications in the Calculus of Variations.* *By* Professor Challis, *M.A., F.R.S., F.R.A.S.**

THE method of integrating differential equations discussed in this communication depends on the following general theorem, which, as far as I am aware, has not been previously enunciated:—

The differential coefficients $\frac{dy}{dx}, \frac{d^2y}{dx^2}, \frac{d^3y}{dx^3}$, &c. being represented by p, q, r, &c., let F be a function of x, y, p, q, r, &c. of any order and degree, and suppose that we have

$$\psi\left(x, y, p, q, \text{\&c. } \mathrm{F}, \frac{d\mathrm{F}}{dx}, \frac{d^2\mathrm{F}}{dx^2}, \text{\&c.}\right)=0,$$

[37] Phil. Trans. Part I. p. 43.
[38] The Mechanics' Magazine, Sept. 8, 1838, vol. xxix. p. 394.
* Communicated by the Author.

the highest order of p, q, &c. not exceeding the highest order of the differential coefficients in F. Suppose also that this equation is satisfied if $F=0$, which equation involves, as is known, the consequences $\frac{dF}{dx}=0$, $\frac{d^2F}{dx^2}=0$, &c. Then any integral of the equation $\psi=0$ which is reducible to an order lower than the order of $F=0$ is to be regarded as an integral of the latter equation. Such integration of a given equation $F=0$ is in principle an extension of the method of integrating by factors, or of that by differentiation, or of a combination of both operations.

Clairaut's form, $y=px+f(p)$, furnishes an instance of integrating by differentiation only. By differentiating we have $q\big(1+f'(p)\big)=0$, which equation is satisfied if $q=0$, and, by consequence, if $p=c$ and $y=cx+c'$. By substituting c for p in the given equation, it will be seen that $c'=f(c)$, so that the proper form of the integral is $y=cx+f(c)$. On eliminating c between this and the derived equation $p=c$, the original equation is reproduced.

For exemplifying integration by a factor let us take the equation

$$\frac{1}{\sqrt{1+p^2}}-\frac{yq}{(1+p^2)^{\frac{3}{2}}}=0.$$

Multiplying by the factor p we obtain

$$\frac{p}{\sqrt{1+p^2}}-\frac{ypq}{(1+p^2)^{\frac{3}{2}}}=\frac{d}{dx}\,\frac{y}{\sqrt{1+p^2}}=0;$$

whence by integration $y=c\sqrt{1+p^2}$, an equation of a lower order by one unit than that of the proposed equation, and consequently a first integral of the latter. A second integration gives

$$x+c'=c\log_e\frac{y}{c}+\sqrt{\frac{y^2}{c^2}+1}=c\log_e(p+\sqrt{1+p^2}),$$

the known equation of a catenary.

If the proposed equation be $1+p^2-qy=0$, it would be made integrable by the factor $\frac{p}{(1+p^2)^{\frac{3}{2}}}$, and the same result as before would be obtained. In fact, differential equations differing in form are one and the same if the factors which make them integrable reduce them to the same form; but no general rule exists for finding the appropriate factor in each case. The foregoing instances come under the general theorem above enunciated, because, if we put F for $\frac{1}{\sqrt{1+p^2}}-\frac{yq}{(1+p^2)^{\frac{3}{2}}}$, the equation

$\psi(x, y, p$, &c., F, &c.$)=0$ becomes in the first case $pF=0$, and in the other $\dfrac{pF}{(1+p^2)^{\frac{3}{2}}}=0$, and each of these vanishes if $F=0$.

For further illustration of the general theorem, I propose now to find the differential equation of a curve every point of which is at a given distance h from the catenary whose equation is obtained above. If x_1, y_1 be the coordinates of any point of the catenary the normal at which is coincident in direction with the normal to the required curve at the point whose coordinates are x, y, we shall have

$$x=x_1+\frac{hp_1}{\sqrt{1+p_1^2}}, \quad y=y_1-\frac{h}{\sqrt{1+p_1^2}};$$

or, because $p_1=p$,

$$x_1=x-\frac{hp}{\sqrt{1+p^2}}, \quad y_1=y+\frac{h}{\sqrt{1+p^2}}.$$

Hence it follows, since $y_1=c\sqrt{1+p_1^2}$, that

$$y+\frac{h}{\sqrt{1+p^2}}=c\sqrt{1+p^2}, \text{ and } c=\frac{y}{\sqrt{1+p^2}}+\frac{h}{1+p^2}.$$

Consequently, by substitution in the equation of the catenary,

$$x+c'=\frac{hp}{\sqrt{1+p^2}}+\left(\frac{h}{1+p^2}+\frac{y}{\sqrt{1+p^2}}\right)\log_e(p+\sqrt{1+p^2}).$$

This is the required differential equation. By substituting $c(1+p^2)-y\sqrt{1+p^2}$ for h, this equation takes the form

$$x+c'=-py+cp\sqrt{1+p^2}+c\log_e(p+\sqrt{1+p^2}), \quad . \ (\alpha)$$

which equation of the first order contains the same constants as the equation of the catenary. It is evident that the resulting equation of the third order would be the same whether c' and h be eliminated by differentiation from the former equation, or c' and c from (α). Adopting the latter process, by differentiating to get rid of c' we obtain

$$0=1+p^2-yq-2cq\sqrt{1+p^2},$$

or

$$2c=\frac{\sqrt{1+p^2}}{q}+\frac{y}{\sqrt{1+p^2}}.$$

Differentiating again to eliminate c, the result is

$$0=\frac{2p}{\sqrt{1+p^2}}-\frac{r}{q^2}\sqrt{1+p^2}-\frac{ypq}{(1+p^2)^{\frac{3}{2}}}, \quad . \ . \ . \ (\beta)$$

which is the differential equation of a curve drawn parallel to a given catenary at an arbitrary distance from it.

Substituting now, as before, F for $\dfrac{1}{\sqrt{1+p^2}}-\dfrac{qy}{(1+p^2)^{\frac{3}{2}}}$, let it be required to find the factors M and N which will make $\mathrm{MF}+\mathrm{N}\dfrac{d\mathrm{F}}{dx}$ identical with the right-hand side of the equation (β). Since

$$\frac{d\mathrm{F}}{dx}=\frac{1}{(1+p^2)^{\frac{3}{2}}}\left(-2pq-yr+\frac{3pq^2y}{1+p^2}\right),$$

the term in $\mathrm{N}\dfrac{d\mathrm{F}}{dx}$ which contains r is $-\dfrac{\mathrm{N}yr}{(1+p^2)^{\frac{3}{2}}}$; and this must be equal to the term $-\dfrac{r}{q^2}\sqrt{1+p^2}$ in (β), no other term containing r. Consequently $\mathrm{N}=\dfrac{(1+p^2)^2}{q^2y}$, and

$$\mathrm{MF}+\mathrm{N}\frac{d\mathrm{F}}{dx}=\mathrm{MF}-\frac{2p}{qy}\sqrt{1+p^2}-\frac{r}{q^2}\sqrt{1+p^2}+\frac{3p}{\sqrt{1+p^2}}$$

$$=\frac{2p}{\sqrt{1+p^2}}-\frac{r}{q^2}\sqrt{1+p^2}-\frac{ypq}{(1+p^2)^{\frac{3}{2}}}\text{, by hypothesis.}$$

$$\therefore\ \mathrm{MF}=-\frac{p}{\sqrt{1+p^2}}+\frac{2p}{qy}\sqrt{1+p^2}-\frac{ypq}{(1+p^2)^{\frac{3}{2}}}$$

$$=\left(p+\frac{2p}{qy}(1+p^2)\right)\left(\frac{1}{\sqrt{1+p^2}}-\frac{qy}{(1+p^2)^{\frac{3}{2}}}\right)$$

$$=\left(p+\frac{2p}{qy}(1+p^2)\right)\mathrm{F}.$$

Hence

$$\mathrm{M}=p+\frac{2p}{qy}(1+p^2).$$

Consequently the equation (β) is equivalent to

$$\left(p+\frac{2p}{yq}(1+p^2)\right)\mathrm{F}+\frac{(1+p^2)^2}{yq^2}\frac{d\mathrm{F}}{dx}=0;\quad .\quad (\gamma)$$

and this equation is verified if $\mathrm{F}=0$, because this supposition involves the consequence $\dfrac{d\mathrm{F}}{dx}=0$. Hence by the general theorem the complete solution of $\mathrm{F}=0$ not only gives a catenary, but embraces all curves that may be drawn parallel to the catenary. These curves, inclusive of the catenary, are plainly all involutes to a common evolute.

It is to be remarked that the integral of the equation (γ), or

its equivalent equation (β), may be considered to be an integral of $F=0$, although the order of the latter equation is inferior by one unit to that of (β). For since the equation $\frac{dF}{dx}=0$ is a necessary consequence of $F=0$, the order of the latter equation may be taken to be the same as that of $MF+N\frac{dF}{dx}=0$. Also the equation (γ) shows that the equation (β) is not satisfied if either $F=0$, or $\frac{dF}{dx}=0$, but by *both* equations.

For another example let

$$y-\frac{a}{\sqrt{1+p^2}}+\frac{ayq}{(1+p^2)^{\frac{3}{2}}}=0.$$

The integration of this equation by means of the factor p gives

$$y^2=\frac{2ay}{\sqrt{1+p^2}}+b^2, \quad . \quad . \quad . \quad . \quad . \quad . \quad (\delta)$$

b^2 being the arbitrary constant. This result does not admit of an exact second integration; but it can be proved that the curve it represents is describable by the focus of an hyperbola which is made to roll on a straight line. Following the same course as in the preceding problem, I shall now proceed to find the differential equation of the first order of a curve drawn parallel to a given curve of the kind just mentioned and distant from it by an arbitrary interval.

Let x_1, y_1 be the coordinates of any point of the given curve, and x, y the coordinates of the point of the parallel curve cut by the normal at the point x_1y_1, or the normal produced. Then, h being the interval between the curves, we have $p_1=p$ and $y=y_1-\frac{h}{\sqrt{1+p_1^2}}$, so that $y_1=y+\frac{h}{\sqrt{1+p^2}}$. Hence, by substitution in the equation (δ),

$$\left(y+\frac{h}{\sqrt{1+p^2}}\right)^2-2a\left(\frac{y}{\sqrt{1+p^2}}+\frac{h}{1+p^2}\right)=b^2.$$

Differentiating to eliminate b^2, and putting for shortness' sake P for $\sqrt{1+p^2}$, the result is

$$\left(y+\frac{h}{P}\right)\left(p-\frac{pqh}{P^3}\right)=a\left(\frac{p}{P}-\frac{ypq}{P^3}-\frac{2hpq}{P^4}\right).$$

To eliminate h this equation is first to be solved as a quadratic with respect to h. It will be found that

$$h^2-h\left(2a+\frac{P^3}{q}-yP\right)=\frac{FP^4}{q},$$

F being substituted for $y - \frac{a}{P} + \frac{ayq}{P^3}$. Hence, putting for brevity R for $a + \frac{P^3}{2q} - \frac{yP}{2}$ and Q for $\frac{P^4}{q}$, we shall have

$$h = R \pm (FQ + R^2)^{\frac{1}{2}}.$$

Hence, differentiating to get rid of h,

$$0 = 2\frac{dR}{dx}(FQ + R^2)^{\frac{1}{2}} \pm \left(F\frac{dQ}{dx} + Q\frac{dF}{dx} + 2R\frac{dR}{dx}\right).$$

Consequently, by squaring to remove the radical,

$$\left(F\frac{dQ}{dx} + Q\frac{dF}{dx}\right)^2 + 4\left(F\frac{dQ}{dx} + Q\frac{dF}{dx}\right)R\frac{dR}{dx} - 4FQ\frac{dR^2}{dx^2} = 0. \quad (\epsilon)$$

This is the differential equation it was required to find; and since, as is readily seen, it is satisfied if both $F = 0$ and $\frac{dF}{dx} = 0$, we may infer, just as in the preceding problem, that the complete integral of the equation

$$0 = y - \frac{a}{\sqrt{1+p^2}} + \frac{ayq}{(1+p^2)^{\frac{3}{2}}}$$

embraces all curves parallel to a certain curve described by the focus of an hyperbola rolling on a straight line. It is plain that the curve itself must be included among those embraced by the integral.

The same result may be obtained by conducting the argument in a somewhat different manner, as follows. If instead of substituting F, R, and Q for the respective functions $y - \frac{a}{P} + \frac{ayq}{P^3}$, $a + \frac{P^3}{2q} - \frac{yP}{2}$, and $\frac{P^4}{q}$, we had operated with the functions themselves, in place of the equation (ϵ) we should have obtained

$$4a^2\left(\left(Pp - \frac{pqy}{2P}\right)q^2 - \frac{P^3r}{2}\right)^2 = P^2p(P^2 + qy)^3(3pq^2 - P^2r). \quad . \quad (\eta)$$

Let us *now* suppose that $F = y - \frac{a}{P} + \frac{ayq}{P^3}$. Then from this equality and its first derived equation it will be found that

$$q = \frac{P^3F}{ay} - \frac{P^3}{a} + \frac{P^2}{y},$$

$$r = \frac{P^3}{ay}\frac{dF}{dx} - \frac{2P^3pF}{ay^2} + \frac{P^3p}{ay} - \frac{2P^2p}{y^2} + \frac{3pq^2}{P^2}.$$

By substituting these values of q and r in the equation (η), and

putting S for $p\left(1-\frac{Py}{2a}\right)$, that equation after due simplifications becomes

$$\left\{\frac{P}{y}\frac{dF}{dx}-\frac{2PpF}{y^2}-\frac{2aS}{y^2}+\left(\frac{PF}{ay}-\frac{P}{a}+\frac{1}{y}\right)^2(PpF+2aS)\right\}^2$$
$$=p\left(\frac{PF}{a}+\frac{2S}{P}\right)^3\left(\frac{P^3}{ay}\frac{dF}{dx}-\frac{2P^3pF}{ay^2}-\frac{2P^2S}{y^2}\right).$$

This is the equation in the present instance which corresponds to the equation $\psi=0$ in the general theorem. Now it is readily seen that this equation is not verified either by $F=0$ or by $\frac{dF}{dx}=0$; but if both $F=0$ and $\frac{dF}{dx}=0$, it is reducible to

$$S\left\{\frac{2SP}{py}+\frac{P^2}{a}-\frac{2P}{y}\right\}=0,$$

which equation is satisfied if $S=0$, and also if

$$S=-\frac{py}{2P}\left(\frac{P^2}{a}-\frac{2P}{y}\right)=p\left(1-\frac{Py}{2a}\right).$$

The supposition that $S=0$ gives $p=0$ and $y=\frac{2a}{P}$, which results (since $p=0$ is satisfied if $y=0$) accord with those derivable from the equation (δ) when $b^2=0$. The other value of S is identical with its assumed value; which proves that the equation (η) is satisfied by $F=0$ and $\frac{dF}{dx}=0$. As the equation (η) is only another form of the equation (ϵ), this result is confirmatory of the inference already drawn from the latter.

The forms of the function F assumed in the last two instances occur in the processes for solving two well-known problems in the Calculus of Variations. In Chapter IV., art. 57, of Mr. Todhunter's 'Researches in the Calculus of Variations' (an Essay which obtained the Adams Prize in 1871), one of these problems is thus enunciated:—"Required the plane curve joining two given points which by revolving round a given axis in its plane will generate a surface of minimum area." After referring to discussions of this problem by several previous writers, and stating that the result given by the ordinary rules of the Calculus of Variations is a catenary of which the axis of x is the directrix, Mr. Todhunter proceeds to examine this solution. He first admits, as established by previous researches, that "sometimes two catenaries can be drawn, sometimes only one, and sometimes none," and then finds, by carrying the investigation further, that "when two catenaries can be drawn the upper corresponds to a

minimum and the lower does not, and that when only one catenary can be drawn it does not correspond to a minimum." According to these results it would seem that the problem does not always admit of a *continuous* solution. To prove that this conclusion cannot be true, it suffices to remark that it is possible to draw through the given points any number of continuous curves which by revolution about the axis would generate surfaces of *different* magnitudes—and that, as the decrement of magnitude from one surface to the next inferior cannot go on unlimitedly, a limiting minimum surface must eventually be reached, the magnitude of which the Calculus of Variations ought to be capable of determining. Consequently there must be some fault or defect in the usual process of solution. It is true, as Mr. Todhunter has pointed out, that a *discontinuous* solution may be deduced from the equation $y=c\sqrt{1+p^2}$; for if $c=0$, and p be not infinite, $y=0$, and, c being zero, if p be infinite y has arbitrary values. Hence the conditions of the problem might be satisfied by the two ordinates to the given points and the portion of the axis of x between them. But this discontinuous solution affects in no manner the validity of the foregoing argument for a continuous solution.

The difficulty thus encountered will, I believe, be found to be completely removed by the new process of integration I have proposed, which rests on the principle that if a differential equation containing F and $\frac{d\text{F}}{dx}$ vanishes when $\text{F}=0$ and $\frac{d\text{F}}{dx}=0$, an integral of that equation of an order lower than that of F is an integral of $\text{F}=0$. In the present instance the function F is

$$\frac{1}{\sqrt{1+p^2}}-\frac{yq}{(1+p^2)^{\frac{3}{2}}};$$

and it has been shown that the equation (β), which belongs to any involute of the evolute of the catenary obtained by integrating $\text{F}=0$, is satisfied if $\text{F}=0$ and $\frac{d\text{F}}{dx}=0$. Consequently, by the rule just stated, such involute is an integral of $\text{F}=0$, and may be employed for solving the proposed problem. On account of the additional arbitrary constant contained in this integral, it is always possible to make the involute pass through the two given points.

The other problem is thus stated in Chapter V., art. 73, of the work already cited:—"To determine a solid of revolution the surface of which is given, so that it may cut the axis of revolution at given points and have a maximum value." The solution I am about to propose would be applicable if the generating

line of the surface terminated at two given points not on the axis; but for the sake of simplicity I shall take the problem as above enunciated.

The history of the various attempts that have been made to solve this problem is very instructive relatively to the principles of the Calculus of Variations, especially as regards the distinction between continuous and discontinuous solutions. With respect to the principle of discontinuity, it may first be remarked that the integral of the equation designated as $M=0$ in Mr. Todhunter's work (the same that I have usually called $A=0$) may either give one line, or two or more lines. But in such cases there will be, according to Algebraic Geometry, a factor corresponding to each line, and the answer to the question may be given by parts of lines terminating at points where two lines cross each at *finite* angles of inclination. This is the case in the solution I gave of the Problem of the Brachistochronous course of a Ship, which is considered by Mr. Todhunter (Chap. I., art. 12) to be the first instance of a solution exhibiting this kind of discontinuity. The problem before us presents another instance of the same kind, inasmuch as we know beforehand that the spherical form is that for which the volume for a given surface is a maximum, and that consequently one solution of the problem should be given by a straight line coincident with the axis of revolution and a semicircle terminating at both ends in that line. It is found in fact that this solution is deducible from the equation (δ) when b^2 is supposed to vanish, in which case $y=0$, which is the equation of the straight line, and $y=\frac{a}{\sqrt{1+p^2}}$, which is the differential equation of the circle. Moreover the differential equation $pM=0$, which requires the factor p for effecting the integration, is satisfied by $y=0$, the equation of the straight line, which causes p to vanish, and by $x^2+y^2=4a^2$, the equation of the circle, which causes M to vanish. This discontinuous solution was given by the Astronomer Royal in the Number of the Philosophical Magazine for July 1861.

Again, as has been already stated, the integral of the equation (δ) gives the curve which is described by the focus of an hyperbola rolling on a straight line. But, contrary to what might have been expected, this curve by no means gives the solution of the problem. It is wholly inapplicable if the positions of the extremities of the curve, whether on or off the axis, be given; but if the abscissæ only of the extreme ordinates be given and the surface generated by the revolution of these ordinates and the curve joining their extremities be also given, the solution shows that the form of this curve is that traced by the focus of a rolling hyperbola, that the extreme ordinates are equal, and

that the connecting curve joins on to the ordinates continuously. The discovery of this discontinuous solution, which differs in kind from that discussed above, is due to Mr. Todhunter.

We have not, however, as yet arrived at a solution which gives a single continuous curve joining the given points—although, from considerations exactly like those adduced in the case of the preceding problem, it is certain that such a solution exists. The difficulty, as in that case, is overcome by employing the proposed new method of integration, which in the present instance consists in first deducing from the equation (δ) the differential equation of the first order of any curve parallel to the curve traced by the focus of the rolling hyperbola, and then showing that the equation (η) resulting from the elimination of the constants from that differential equation is verified if $\mathrm{F}=0$ and $\frac{d\mathrm{F}}{dx}=0$, the function F being $y-\frac{a}{\sqrt{1+p^2}}+\frac{ayq}{(1+p^2)^{\frac{3}{2}}}$. The parallel curve, as I have already argued, is under these circumstances an integral of $\mathrm{F}=0$; and since its equation contains one more arbitrary quantity than the equation of the curve itself, it is capable of satisfying the given conditions of the problem. I have ascertained, for instance, that if the given surface be of small amount for a considerable distance between the given points on the axis, the form of the curve is like the arc of a bow; but if the amount of surface be large for a small interval between the points, the curve approaches the circular form. Under the supposed circumstances such forms might be antecedently expected to be given by the results of the analytical calculation.

The foregoing solutions of the two problems do not essentially differ from those I proposed in an article contained in the Philosophical Magazine for July 1871. But the latter solutions were effected by actually finding the equations of the evolutes corresponding to the two series of involutes; whereas the present research has shown that this process was not necessary, and that the new integration is virtually an extension of the recognized methods of integrating by factors and by differentiation. The principle of the application of this new method in these two instances may be briefly stated as follows. A differential equation $\mathrm{F}=0$ of the second order may have an integral containing *three* arbitrary constants, but only on the condition that the differential equation of the third order obtained by eliminating these constants from the integral is verified by the *two* equations $\mathrm{F}=0$ and $\frac{d\mathrm{F}}{dx}=0$, and not by either singly. The reason is, that as the equation $\frac{d\mathrm{F}}{dx}=0$ is a necessary consequence of $\mathrm{F}=0$, the

latter equation, in case the above condition can be fulfilled, is really of the third order.

I have now reached the conclusion of my researches in the Calculus of Variations, having succeeded at length in removing from its analysis the reproach of failing to solve a class of problems which unquestionably should thereby be capable of solution. For instance, it is obvious that there must be a continuous surface of revolution which for a given amount of surface and a given length of the axis encloses a maximum volume. But so far was this problem from being solved, that, on account of its peculiar difficulties, those who from their reputation as analysts might be expected to furnish a solution, have only endeavoured to show that it is insolvable. Probably, therefore, it will be held to be excusable that in my first attempts to solve it I adopted views and processes of reasoning which I had afterwards to abandon as untenable. I persevered, however, in my efforts to arrive at the solution, not being able to admit the possibility of the failure of analysis; and the results now communicated I consider to be a justification of this course.

Cambridge, October 8, 1873.

L. *On Statical and Dynamical Ideas in Chemistry.*—Part IV. (*conclusion*). *On the Idea of Motion.* *By* Edmund J. Mills, *D.Sc.**

Contents.

A criterion of scientific progress is needed. This criterion is an idea common to all the sciences—namely, the idea of pure motion. It was first announced by Herakleitos of Ephesus; we afterwards trace it through the Platonic Sokrates, the Sophists, Aristotle, the Epicureans, and the Skeptics. It reappears in Hobbes and Hegel. Ferrier and Spencer compared with reference to this idea. All conscious knowledge is but knowledge of motion. The science of the naturalist, mathematics, and geology are illustrations of the directive force of the idea. In chemistry—which has not, like those sciences, assumed a "new" phase—a theory of absolute limits prevails; but its great epochs are intelligible only by the light of this criterion: its imaginary pursuits; study of the chemical process neglected. The extinction of ethics. Questions. Summary. Practical tendency of the discussion.

Ποταμοῦ ῥοῇ ἀπεικάζων τὰ ὄντα.

If a chemist, in the course of his researches, were to suspend for a moment his more immediate investigation and ask himself *Is this Progress?* he would have a difficult question to answer. I do not think it advisable that this question should often be asked, or that every chemist should ask it. But it does arise in the most legitimate manner; and, as it has a tran-

* Communicated by the Author.

scendant importance in the economy of intellectual effort, an answer is imperatively required. Moreover, as in every science the same demand springs up, how vast must be the interests concerned, how great the value of a right response!

The question stated presupposes that a deficiency or a requirement does actually exist—in other words, that the science from which the question comes in some respect differs from the general scientific level. Now in Part I. of these papers*, I pointed out what is the universal criterion of progress—namely, "the most general idea existing at a given time as a factor in every branch of science." My position may be illustrated by an experimental usage. Suppose a phenomenon has to be observed by reading off a number; this is done, not once, but many times, and the average result is calculated. The average is certainly more valuable than either of the observations taken singly; and, if these are regarded as combining towards an end, it is contained to a greater or less extent in each of them. The idea of pure motion is asserted to be the mean or general idea sought. I now propose to explain more fully in what sense this assertion is made, and to discuss more at large than was possible in Parts II. and III. the nature of the criterion itself.

The general nature of the criterion establishes its universal applicability. But I address myself more especially to chemists; and their science is at the present time, and at this instant to the judicial reader, in the very crisis when the adoption of a real criterion is of the supremest consequence. Sad in the poverty of symbolic resources, miserably unreasoned and deficient in power, it urgently requires an entire reform of its prevailing theory. The student more especially, and the teacher (who is in each instruction a necessary artificer of prejudices), have need to pause, and find or verify that "*word*," which, like the κριτικὸς λόγος of St. Paul†, is to divide and penetrate all the problems of knowledge.

The history of the idea of pure motion is comparatively brief and simple. Herakleitos of Ephesus (460 B.C.), who first announced it, had clearly seen its vast importance and universal applicability; and, though termed by his successors The Dark (Σκοτεινός), he was undoubtedly understood by many of them. The Sokrates of Plato‡ seems on the whole, to accept Herakleitos's doctrine§ and frequently alludes to it; where he treats

* Phil. Mag. S. 4. vol. xxxvii. p. 461.

† Hebrews, chap. iv. verses 12, 13.

‡ In the *Theætetus*.

§ Ferrier (Lectures on Greek Philosophy, vol. i. pp. 145, 146) writes of Herakleitos, "here, if anywhere, is the embryo of the solution of the enigma of the universe. I am convinced that the unity of contraries is the law of

it humorously, the sport is rather at the expense of his auditors' intelligence than concerning his subject. Herakleitos pointed out the illusory nature of the seeming permanence or stationary condition of things: "every thing moves, and nothing remains," he said. The flow of a river is an apt illustration of the fleeting nature of the world; in which, indeed, the only reality is the act of transition, or the "becoming;" not inception and results, but process. In the instant of "becoming" he conceives the contrary determinations of being and not-being united,—a notion which Professor Ferrier has beautifully illustrated by the well-known geometrical explanation of two opposite forces combining in a continuous curvilinear path. The sophists, especially Protagoras, applied this doctrine, though not with the highest success, to justice and morals, chiefly in the interest of the freedom of the individual will. In the Aristotelian philosophy, the conditions of nature are summed up as *motion, space* (the possibility of motion), and *time* (the measure of motion); but, in Aristotle, the idea becomes less pure, is circumscribed with limits in the detail, and loses in elasticity and vigour. The same is true of the Epicureans; but it formed the latent basis of their philosophy, as likewise of the Skepticism of every age.

In much more modern times, the idea of motion was most distinctly grasped by Hobbes, as may be seen by referring to his 'Humane Nature,' 2nd edition (1650), from which work I have taken the following statements. "That the Subject wherein

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all things—that all life, all nature, all thought, all reason centres in the oneness or conciliation of Being and not-Being. A firm grasp of this doctrine, a clear insight into its truth, and a vigorous enforcement of it and its consequences, would lead to the construction of a truer philosophy than that which is at present so much in vogue. That philosophy is founded entirely on the denial of the unity of contrary determinations in the same subject. It takes two opposite conceptions, and holding them apart it shows that reason is baffled in its attempts adequately to conceive either of them. It is in this way that Sir W. Hamilton and Mr. Mansel achieved what they conceive to be a great triumph in proclaiming, or, as they think, in proving the impotency of human reason. But what if the conceptions thus set in opposition to each other are not conceptions at all, but are mere moments or elements of conception?" Compare with this the following passage from Herbert Spencer (First Principles, 2nd edit. p. 277). "The law we seek, therefore, must be the law of *the continuous redistribution of matter and motion.* Absolute rest and permanence do not exist. . . . . And the question to be answered is—What dynamic principle, true of the metamorphosis as a whole and in its details, expresses these ever-changing relations? . . . . . a Philosophy rightly so-called can come into existence only by solving the problem." And again, p. 285, "While the general history of every aggregate is definable as a change from a diffused imperceptible state to a concentrated perceptible state and again to a diffused imperceptible state, every detail of the history is definable as a part of either the one change or the other. This, then, must be that universal law. . . . ."

Colour and Image are inherent, is *not* the *Object* or thing seen. That there is nothing *without us* (really) which we call an Image or Colour. That the said Image or Colour is but an *apparition* unto us of the *motion,* agitation or alteration which the *Object* worketh in the *Brain,* or spirits, or some internal substance of the head. That as in Vision, so also in conceptions that arise from the *other Senses,* the subject of their inherence is not the *Object,* but the Sentient." (p. 9.)

Hobbes also says that the reflex stroke from the brain to the impressing object constitutes our *sense* of colour, form, sound, etc. What is really outside is "*motions,* by which these seemings are caused." (p. 18.)

"*Conceptions* and *apparitions* are nothing *really,* but motion in some internal substance of the head." (p. 69.)

"All *evidence* is *conception* . . . . and all conception is imagination, and proceedeth from *Sense.*" (p. 135.)

Hobbes does not seem to have anywhere definitely stated the final conclusion deducible from the above propositions. But they involve a sorites, the beginning of which is the mental act, the end of which is motion: motion is therefore exclusively our being. The argument may be exhibited as follows. I only know of events by sensations, which can only be represented as motion; the organs or parts of my body whereby I know these are themselves events, both to me and other intelligences. All knowledge of events is therefore knowledge of motion. In "events" are included the emotions, will, and intellect of other persons; consequently my own. A moving body is merely directed or relative motion. This theory must apply at least to all conscious knowledge*.

The last philosopher who can be said to have advocated the idea of pure motion was Hegel. In his dialectical method, which always advances "from notion to notion through negation"—in his doctrine of being, as always (pendulum-like) vanishing into nothing and back again—in these and other principal features of Hegel's system, we observe, as its author acknowledged, the restoration of the Herakleitic principle. It would be a mistake, however, to suppose that Hegel, in endeavouring to trace out with much minuteness the universal prevalence of his dialectic, agrees at all points with his Grecian predecessor; but, on the whole, his fundamental idea is most convincingly the same.

The philosophic aspect of the idea of pure motion, as portrayed in the preceding paragraphs, indicates the prevalence of that ideal influence from very early historic times until now; and

* Further expositions of Hobbes's view will be found in his work 'The Leviathan' (1651), pp. 3, 352, 369, 374, 375.

shows, moreover, how natural it is and must be to man's mind to acquire. Great thoughts, however, do not remain for ever without fruit, even when held unconsciously by ignorant people. Secret and imperceptible, they grow. Hence it becomes important to ascertain whether there are any decided practical evidences of the idea of pure motion in modern science.

The science of the naturalist had always proved dark and difficult whenever an attempt was made to transcend facts and verge upon principles. Its valuable functions were those of a warehouseman and clerk—to collect and register; its speculations hovered over the facts rather than rested upon them. The light but pungent satire of Reybaud must be admitted to have had its point. "Celui-ci, me disait-il, appartient corps et âme aux entomozoaires; il a eu la chance de découvrir une quinzième articulation dans un insecte, et des antennes que personne n'avait soupçonnées avant lui . . . . Il passera à la posterité avec son hymenoptère, sans compter une espèce de scolopendre qui lui a de grandes obligations. Supprimez cet homme de la communauté humaine, et voilà des scolopendres qui n'occupent pas, dans l'échelle des êtres, le rang qui leur appartient. Lui seul a pu en faire huit genres, douze sous-genres, sans compter les variétés." Wide as was the naturalist's scope, his chief duty was the discovery, description, and arrangement of species—species believed, for the most part, to have been distinctly created, and to be each one of them an instance of a break in nature. But the subtle principle of motion, altogether opposed to stationary points, or discontinuity, can now be seen to have been ever asserting itself, asserting itself in more or less fanciful theories of archetypes, of the emanations of beings from few sources, of a fundamental identity of origin for animals and plants*. At length, with seeming but not veritable suddenness, the beautiful and harmonious hypothesis of Darwin and Wallace arose, and ere long was found to comprehend within its grasp the varied forms of the world's life. For many years to come it will be the finest task of the naturalist to work at the verification of that hypothesis, though we have even now the present value of such verification. Darwin's theory is familiar to every one; the principle of it may be gathered from the following statements of its originator †:—"A French author, in opposition to the whole tenor of this volume, assumes that, according to my view, species undergo great and abrupt changes, and then

* It remains for the philosophic chemist and physicist to bridge over the artificial barrier between "living and non-living matter," and to prove that life is the common property of all sensuous objects.

† On the Origin of Species by means of Natural Selection (1866), pp. 146, 232, 246.

he triumphantly asks how this is possible, seeing that such modified forms would be crossed by the many which have remained unchanged."

"Why should not nature take a sudden leap from structure to structure? On the theory of natural selection we can clearly understand why she should not; for natural selection acts only by taking advantage of slight successive variations; she can never take a sudden leap, but must advance by short and sure though slow steps."

"On the theory of natural selection we can clearly understand the full meaning of that old canon in natural history, 'natura non facit saltum.' This canon, if we look only to the present inhabitants of the world, is not strictly correct; but if we include all those of past times, whether known or not yet known, it must by my theory be strictly true."

Here, then, we have, stated in the clearest language, explicit evidence that the great principle acquired by the naturalist, and now governing the entire extent of his observations, is continuity, a derivative form of the idea of motion.

I turn to the science of algebra. It, too, has gone through a "new" phase, and now exists under conditions which are most remarkable, though these conditions have been developed in the most gradual and orderly manner from the time of Newton, as by him from his predecessors. It, too, is morphological; through it every branch of modern mathematics has again become young. The interoperation of algebraic forms corresponds to those conflicting conditions of nature by which species are produced and maintained. But what sun was it that gave such light to the mathematician? The same that shone on that other continent of the naturalist. "Time was when all the parts of the subject were dissevered*, when algebra, geometry, and arithmetic either lived apart or kept up cold relations of acquaintance confined to occasional calls upon one another; but that is now at an end; they are drawn together and are constantly becoming more and more intimately related and connected by a thousand fresh ties; and we may confidently look forward to a time when they shall form but one body with one soul. Geometry formerly was the chief borrower from arithmetic and algebra; but it has since repaid its obligations with abundant usury; and if I were asked to name, in one word, the pole-star round which the mathematical firmament revolves, the central idea which pervades as a hidden spirit the whole corpus of mathematical doctrine, I should point to Continuity as contained in our notions of space, and say, it is this, it is this!"

* Sylvester, British Association's Report (1869), Transactions of Sections, p. 7.

In the antagonism between continuity in mathematics and alleged absolute limits in chemistry, we see the reason why so few chemists are mathematicians, and so few mathematicians chemists.

Geology has, in recent years, shaken itself free from the fetters of the cataclysmal school, and finds in existing forces, continuously exerted, an adequate explanation of her special phenomena.

Chemistry still looks with half-averted face upon all dynamical doctrines. But her great centres of historic conflict are intelligible only by their aid. Acid, Alkali, Base, and Salt are not capable of definition as particular things*; the principle of continuity alone renders them clear. Chemical Substance is homogeneous, not discontinuous substance; Chemical Functions are modes of motion†. The Atomic Theory, triumphant still, is more suspected than before; but it is indeed a better servant to pure dynamics; for it places before the mind, daily and most distinctly, the fatal consequences of the assumption that quantity consists of parts‡. Grave and mature chemists now investigate the position of a particular atom in an aromatic compound, and find it at the side, in the middle, or near some other portion of an open or closed chain. In the mean time we hear nothing of the chemical process. Such are our dreams; we think them so regular, orderly, and reasonable. And so they are, until the morning; *then* we shall be the first to laugh at ourselves. For my own part, the perusal of modern chemical literature fills me with an abiding sense of sorrow and shame.

In the practical side of philosophy the idea of motion, in various derived forms, is also found ascendant. It is for the most part to philosophers that the intellectual bias of modern nations is due; and the characteristics of contemporary society, having been so derived, are in the main dynamical. The sentiments, the affections, the passions of mankind have been flooded with liberty and power. It has been found advisable, and even necessary, to remove many of the older educational restraints, and to qualify at least the social distinctions we preserve. The science of ethics has expired, with all necessity for formal sanctions. The notions of right and wrong can now be derived historically, and are no longer mysterious. While Idealism has, on the one hand, restored to its supreme and lawful rank the individual Self, it should be the business of the schools to make men know that; in the mean time, the wide demand for liberty of the will is not only audible but imperative. But it is evident that, as soon as the ethical sanction is defined as liberty or free-

* Phil. Mag. S. 4. vol. xxxvii. p. 461.

† Ibid. S. 4. vol. xl. p. 259.

‡ Ibid. S. 4. vol. xlii. p. 112.

dom, no sanction any longer exists; act and motive are identical. This strange phenomenon of the destruction of a science has been witnessed by the present generation; it is as rare in human history as the extinction of a star.

With regard to other departments of practical philosophy, I need merely state the names of politics, religion, and sociology, and leave it to the candid reader to consider whether their nearer history has been more characterized by freedom or definition. To his judgment I also relinquish the questions whether liberty, toleration, the scientific spirit, and the growth of all our noblest interests, are not more likely to be fostered by cultivating the great standard I have discussed than by the dogma of an absolute limit. Where can we find "contentment, but in proceeding"*? or peace, but in action? or hope, but in the drift of nature?

Thus, then, having reflected upon the principles of our daily actions and the more recondite sources of our scientific life, we see rising from them all, like a spirit out of darkness, the perfect majesty of motion. This is the centre, this is the vast and ever-receding circumference of fulfilled desire. All beauty, all life, all thoughtful power, are contained in it and are it. Like a river it has glided on, silent, devious, unperturbed. From a Grecian source toward the untravelled sea the stately ships of science take freight and spread their sails upon its bosom; the gilded craft of music, of poetry, and of rhetoric disport there; and the tiny barks of incipient civilization have but that single channel. It has drained, refreshed, and fertilized every continent of thought and feeling. At a distance from its margin are chiefly found the hideous desert and the dry rocks of old dogmatic conflict; but nearer to the shore come soft oases; and its banks are fertile vales, content, and sweet and tranquil. All remote inspirations, all scattered fragments of knowledge, are but dew or clouds exhaled at first from its surface, or wafted from its waves.

Ah, Shade of Herakleitos! leave his urn, and weep no more; for the teachings of that mighty mind are not only understood, but have been verified, and we can now coordinate them from new forms. Henceforth it remains but to apply his great idea, now seen to be the widest generalization from experience—to turn round, and, journeying backward into detail, take that as both map and compass on the road. Henceforth, for us who have chosen the criterion, all that is good and desirable is motion, all that is evil and to be dreaded is a limit. Let no one strive to reconcile them.

* Hobbes.

12 Pemberton Terrace,
St. John's Park, N.

LI. *Notices respecting New Books.*

An Elementary Treatise on the Differential Calculus, containing the Theory of Plane Curves, with numerous Examples. By BENJAMIN WILLIAMSON, *A.M., Fellow and Tutor, Trinity College, Dublin.* Second Edition, Revised and Enlarged. London: Longmans, Green, and Co. 1873. (Pp. 367.)

WE noticed the first edition of this book, shortly after its publication, about a year and a half ago (April 1872). There is therefore no need to say much on the present occasion. The revision to which the work has been subjected consists partly in a change of arrangement of some of the articles (*e. g.* the chapter on Lagrange's Theorem, which was Chap. 20 in the first edition, is Chap. 7 in the second), partly in the addition of articles here and there throughout the volume (*e. g.* the articles on Linear Transformation, Nos. 292–294 added to the chapter on Change of the Independent Variable), partly in the insertion of additional Examples. The total number of changes is considerable, and the result of the whole is to increase the volume by 24 pages; still the changes do not make what can be regarded as a substantial alteration in the work. It only remains, therefore, to congratulate the author on the rapid sale of the first edition, and to express a hope that his very useful book will continue to find many readers.

LII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 326.]

May 1, 1873.—William Spottiswoode, M.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On the Effect of Pressure on the Character of the Spectra of Gases." By C. H. Stearn and G. H. Lee.

The variations in the spectra of gases which accompany changes of density have been studied by Plücker and Hittorf, Frankland and Lockyer, Wüllner and others.

It appears to us that one cause to which these changes may be due has been overlooked, and that many of the observed variations are entirely independent of the density of the gas. If a Leyden jar be placed in the circuit, and the current from an induction-coil be passed through a Plücker's tube containing nitrogen with the traces of hydrogen generally present, the following well-known phenomena are observed.

When the gas is near atmospheric pressure, the line-spectrum of nitrogen is brilliant, and the F line of hydrogen is broad and nebulous. As the pressure is reduced, the lines of nitrogen gradually fade out, and the band-spectrum appears, while at the same

time the F line of hydrogen becomes narrow and well defined. If fresh gas be admitted, the line-spectrum reappears, accompanied by a widening of the F line.

That these changes are not dependent on the density of the gas, appears from the following experiment:—

A sealed tube containing nitrogen, with traces of hydrogen at a pressure of about 2 millims., was placed before the spectroscope. A second tube was connected with the air-pump, and the current passed through both tubes, a Leyden jar being placed in the circuit.

When the pressure in the second tube was high, the line-spectrum of nitrogen appeared brilliantly in the sealed tube, and the F line was broad and nebulous; as the exhaustion proceeded these lines faded out, and the F line became narrow, in precisely the same manner as if the sealed tube had been in process of exhaustion. The explanation appears to be that the production of the line-spectrum of nitrogen, and the expansion of the F line of hydrogen, depend entirely on the intensity of the charge communicated to the Leyden jar. When the pressure of the gas between the electrodes is high, the discharge does not take place until the jar is fully charged; but as the exhaustion proceeds a less and less charge is communicated to the jar, and the discharge at last is virtually not more than that of the simple current.

The same effect may be produced by interposing a break in the circuit, the length of which may be increased as the pressure in the tube is reduced. Plücker and Hittorf appear to have used a break, as in their paper in the Philosophical Transactions, Nov. 1864, they speak of the expansion of lines obtained by increasing the charge of the jar by an interposed stratum of air. They do not, however, appear to have noticed that the reduction of pressure in the tube was only equivalent to a diminution of the charge of the jar, and that to this cause many of the changes of spectra which accompany the reduced pressure ought to be ascribed.

We are continuing our experiments on the effect of temperature on the spectrum, but prefer to reserve this portion of the subject for the present.

May 8.—Francis Sibson, M.D., Vice-President, in the Chair.

The following communication was read:—

"Researches in Spectrum-Analysis in connexion with the Spectrum of the Sun."—No. II. By J. Norman Lockyer, F.R.S.

The observations in this paper are a continuation of those referred to in the previous communication bearing the same title. They deal (1) with the spectra of chemical compounds, and (2) with the spectra of mechanical mixtures.

I. *Chemical Compounds.*

Several series of salts were observed; these series may be divided into two:—1st, those in which the atomic weights varied in each

series; 2nd, those in which the associated elements varied in each series. The following salts were mapped:—

$Pb\,F_2$, $Pb\,Cl_2$, $Pb\,Br_2$, $Pb\,I_2$; $Sr\,F_2$, $Sr\,Cl_2$, $Sr\,Br_2$, $Sr\,I_2$; $Ba\,F_2$, $Ba\,Cl_2$, $Ba\,Br_2$, $Ba\,I_2$; $Mg\,F_2$, $Mg\,Cl_2$, $Mg\,Br_2$, $Mg\,I_2$; $Na\,F$, $Na\,Cl$, $Na\,Br$, $Na\,I$.

The conditions of the experiments are described. The same aluminium cups, described in the first paper, were used; and the poles were arranged in such a manner that they could at will be surrounded with any gas or vapour. Hydrogen was used in some of these experiments; it was purified in the usual manner by drying and freeing from traces of sulphuretted hydrogen; it was then passed over clean-cut pieces of sodium, and admitted to the poles. An induction-spark from 5 one-pint Grove cells was used, the *circuit being without the Leyden jar.*

The lead compounds behaved (in air) as follows:—

The fluoride gave the eleven longest lines of the metal; but four were very faint.

The chloride gave nine lines; one of these was very short.

The bromide gave six lines; but one was a mere dot on the pole.

The iodide gave four lines distinctly and two as dots, one of which was scarcely visible.

It is pointed out that the decrease in length and number of lines follows the increase in the atomic weight of the non-metallic element, the lines dying out in the order of their length.

Barium was next experimented on, the same series of salts being used. A marked departure from the results obtained in the case of the lead compounds was observed, especially in the case of the fluoride, its spectrum being much the simplest; in fact it consisted of only 4 lines. Strontium behaved like barium; and so did magnesium fluoride. This anomalous behaviour was found to be most probably due to the exceedingly refractory nature of these fluorides, all of them being quite infusible, and non-volatile in any spark that was used.

Sodic fluoride, sodic chloride, sodic bromide, and sodic iodide exhibited a behaviour exactly the reverse of that of lead; *i. e.* the iodide showed most of the metallic spectrum.

The difference between flame-spectra and those produced by a weak electric discharge are then discussed. Beads of the chlorides &c. were heated in a Bunsen gas-flame. BaI_2 gave a "structure" spectrum (since proved to be due to the oxide) and the line at wave-length 5534·5, by very far the longest metallic line of barium. The bromide behaved like the iodide; and so did the chloride, except that its spectrum was more brilliant. Baric fluoride gave scarcely a trace of a spectrum, the oxide structure being scarcely visible, and 5534·5 very faint indeed. The strontium salts follow those of barium—4607·5, the longest strontium line, appearing in conjunction with an oxide spectrum. The strontic fluoride, however, refused to give any spectrum whatever. These

results are compared with those obtained with the weak spark; and it is shown that the difference is one of degree: *e. g.* baric bromide gives 25 lines in the spark; these are the longest lines. In the flame it gives but one line; but this is the longest of all the barium lines, and indeed very far exceeds all the others in length. When the flame-spectra are compared with those produced by the low-tension spark, the spectra of the metals in the combination are in the former case invariably more simple than in the latter, *so that only the very longest line or lines are left.*

Some experiments made by Mr. R. J. Friswell to determine the cause of the similarity of the spectra of the various salts of the same metal observed in air are then given, the conclusion being that the spectrum observed is really that of the oxide.

Kirchhoff and Bunsen's, Mitscherlich's, and Clifton and Roscoe's prior conclusions on the points investigated are stated at length; and it is shown that the observations recorded, taken in conjunction with the determination of the long and short lines of metallic vapours, are in favour of the views advanced by Mitscherlich, Clifton, and Roscoe. For while the spectra of the iodides, bromides, &c. of any element in air are the same, as stated by Kirchhoff and Bunsen, the fact that this is *not* the spectrum of the metal is established by the other fact, *that only the very longest lines of the metal are present, increased dissociation bringing in the other metallic lines in the order of their length.*

The spectra have been mapped with the salts in hydrogen: here the spectra are different, as stated by Mitscherlich; and *the metallic lines are represented according to the volatility of the compound, only the very longest lines being visible in the case of the least-volatile one.*

The following are the conclusions arrived at:—

1. A compound body has as definite a spectrum as a simple one; but while the spectrum of the latter consists of lines, the number and thickness of some of which increase with molecular approach, the spectrum of a compound consists in the main of channelled spaces and bands, which increase in like manner. In short, the molecules of a simple body and of a compound one are affected in the same manner by their approach or recess, so far as their spectra are concerned; *in other words, both spectra have their long and short lines or bands.* In each case the greatest simplicity of the spectrum depends upon the greatest separation of molecules, and the greatest complexity (a continuous spectrum) upon their nearest approach.

2. The heat required to act upon a compound, so as to render its spectrum visible, dissociates the compound according to its volatility: the number of true metallic lines which thus appear is a measure of the dissociation; and doubtless as the metal lines increase in number the compound bands thin out.

Mitscherlich's observations, that the metalloids show the same structural spectra as the compound bodies, is then referred to, and

the question is asked whether the molecules of a metalloid do not in structure lie between those of elements on the one hand and of compounds on the other.

These considerations are applied to solar and stellar spectra. The general appearance of the solar spectrum shows that in all probability there are no compounds in the sun.

Secchi's maps of a large number of stellar spectra are referred to as now indicating beyond all doubt the existence of compound vapours in the atmosphere of some stars; and it is suggested that the phenomena of variable stars may be due to a delicate state of equilibrium in the temperature of a star, which now produces the great absorption of the compound and now that of the elemental molecules.

II. *Mechanical Mixtures.*

The second part of the paper deals with the mechanical mixtures. Maps of the spectra of alloys of the following percentages are given:—

Sn and Cd	percentages of Cd	10·0, 5·0, 1·0, 0·15.
Pb and Zn	,, ,, Zn	10·0, 5·0, 1·0, 0·1.
Pb and Mg	,, ,, Mg	10·0, 1·0, 0·1, 0·01.

It is pointed out that the lines disappear from the spectrum as the percentage becomes less, the shortest lines disappearing first —and that although we have here the foreshadowing of a quantitative spectrum-analysis, the method is so rough as to be inapplicable.

It is then stated that further researches on a method which promises much greater accuracy are in progress.

The bearing of these results on our knowledge of the reversing layer of the sun's atmosphere is then discussed.

LIII. *Intelligence and Miscellaneous Articles.*

ON THE CONDENSATION OF GASES AND LIQUIDS BY WOOD-CHARCOAL. THERMIC PHENOMENA PRODUCED ON THE CONTACT OF LIQUIDS AND CHARCOAL. LIQUEFACTION OF THE CONDENSED GASES. BY M. MELSENS.

ABSORPTION of chlorine by wood-charcoal may go on until it represents a weight of chlorine equal to that of the charcoal; consequently the condensing force of the latter may serve to realize the liquefaction of the non-permanent gases.

Charcoal put into a tube similar to Faraday's Λ-shaped tube is saturated with chlorine. The two extremities of this siphon tube being then sealed at the lamp, if the long branch of the tube be heated in a water-bath of boiling water, and the short branch be dipped into a freezing-mixture, a considerable quantity of chlorine leaves the charcoal and resumes the gaseous state; and under the

influence of the pressure developed, this gas liquefies in the cooled short branch.

I have in this way obtained several cubic centimetres of pure liquid chlorine. On taking the tube out of the bath, the liquid chlorine commences spontaneous ebullition, and again condenses on the charcoal, while the short branch becomes covered with a frost.

This succession of phenomena can be reproduced, so to say, indefinitely; and the experiments, easy to perform at public lectures, permit the audience to observe the various phases.

Although I can only consider my experiments a trial, I have extended them to the liquefaction of several gases absorbed by the charcoal when cold and disengaged by a temperature not rising above 100° C.:—chlorine, ammonia, sulphurous, hydrosulphuric, and hydrobromic acids, chloride of ethyle, and cyanogen. The liquefaction of each of these gases can be demonstrated in lectures when explaining the history of those bodies.

Reflecting on the feeble thermic effects ascertained by Pouillet when pulverulent mineral matters are soaked with water, oil, alcohol, or acetic ether, and on the somewhat greater effects exhibited when the same liquids are absorbed by organized substances, I asked myself if we could not succeed in ascertaining pronounced thermic effects by placing in contact with cellules of charcoal liquids which do not act upon it—water, alcohol, ordinary ether, sulphide of carbon, and bromine.

The experiments exceeded my expectation. For example, with 1 part of charcoal and from 7 to 9 parts of liquid bromine, the rise of temperature exceeds 30° C., operating on only from 5 to 10 grammes of charcoal.

With charcoal well freed from gas, heated and cooled *in vacuo*, the heating due to the imbibition of bromine would doubtless be much more considerable.

The volatile liquids condensed in the pores of the charcoal (bromine, cyanhydric acid, sulphide of carbon, ordinary ether, and alcohol) are not expelled, or only partially, by a temperature of 100° C. at the ordinary pressure. I made the experiment with a Faraday tube, operating as described for the liquefaction of the gases. A tube filled with charcoal saturated with alcohol does not permit any to distil at 100°.

[The tubes were exhibited to the Academy; and with them the principal experiments (the liquefaction of chlorine, cyanogen, &c.) have been repeated in the laboratory of the École Centrale.

The condensation of liquid bromine by charcoal, effected upon a few grammes, gave rise to a brisk rise of temperature, the mixture passing in a few minutes from 20° to 45°.]—*Comptes Rendus de l'Académie des Sciences*, October 6, 1873.

NOTE ON THE POSSIBLE EXISTENCE OF A LUNAR ATMOSPHERE. BY E. NEISON, ESQ.

Owing to the many difficulties with regard to the constitution of

the lunar surface involved in an assumption as to the absolute non-existence of a lunar atmosphere, it would appear of far greater probability that some such atmosphere, however limited, exists. Not only, as Dr. De La Rue has remarked, is it difficult to conceive any chemical formation of matter without an atmosphere, but it is also difficult to even find matter exhibiting the features and properties of that constituting the lunar surface, which under the known conditions would not either yield an atmosphere, or require for formation the presence of substances that would.

The absolute absence of any atmosphere has never yet been demonstrated, but only the fact that it does not exceed certain limits, generally supposed much more restricted than is actually the case. In consequence it is usually granted that some atmosphere might exist; it is also assumed that it must be of most extreme tenuity; and the subject is dismissed as a matter of indifference, without inquiring whither the admission might carry us, so far as relates to this atmosphere's power of fulfilling the same purposes as our own terrestrial one.

But it would be of interest to ascertain how far this possible lunar atmosphere might not effect for the lunar surface those changes &c. that our own does for the terrestrial surface, and whether in fact it might not amply suffice for maintenance of at least some form of vegetable life. For the present, however, this must be deferred.

The only point restricting the extent of a lunar atmosphere of the nature supposed appears to be its refractive power, more especially as shown by the occultation of stars by the moon. Irrespective of the circumstance that these do not invariably answer conclusively in the negative, it does not appear to be generally recognized that we may have an atmosphere whose maximum power of refraction would not be equal to one second of arc, and yet be of very considerable amount. For, of however great tenuity in comparison with our dense terrestrial atmosphere, it would be in reality present in large quantity—to be estimated in fact, with regard to each square mile of surface, by very many thousands of tons.

There can be but little doubt but such an atmosphere would exert a very considerable influence on the lunar surface, render possible the existence of many substances that appear to constitute a great portion of that surface, and explain many selenographical observations of great interest that at present appear to point to some such solution, and thus support the hypothesis of the existence of a definite lunar atmosphere.—*Monthly Notices of the Royal Astronomical Society*, June 1873.

A CONTRIBUTION TO THE HISTORY OF THE HORIZONTAL PENDULUM. BY PROF. ŠAFAŘÍK.

As it is for the most part only some time after date that I get a sight of scientific journals that do not treat of my special department (chemistry), I first became acquainted recently, through the

Philosophical Magazine, with M. Zöllner's great memoir "On the Origin of the Earth's Magnetism, and the Magnetic Relations of the Heavenly Bodies." In § 24 of this (like every thing that comes from M. Zöllner) ingenious and thoughtful treatise, there is a detailed account of a new instrument, which in November 1869 had been brought by M. Zöllner to the notice of the Saxon Royal Society of Sciences, in a memoir "On a new Method for measuring Attracting and Repelling Forces"—which had remained unknown to me. He proposes to name the new instrument "horizontal pendulum," mentioning also that, in 1863, M. Perrot had proposed for the same purposes, and described in the *Comptes Rendus* of the Paris Academy, an instrument resting on the same principles.

It is of high interest, and will certainly interest M. Zöllner to learn, that, more than a generation since, in Germany his instrument was not only described and figured, but also applied to experiments, although no particulars of the results are given.

Singularly, the matter goes back to a man whose name has no good sound in connexion with the exact sciences, and from whom it certainly would be least expected—Gruithuisen of Munich. I feel so much the more obligation to render him this late justice, as seven years since, in a longer memoir published in the *Böhmische Museumzeitschrift*, vol. xxxix., "On the present State of Lunar Investigations," I took the fanciful philosopher of Munich somewhat sharply to task—in fact, designated him as selenoprotophantast. I feel now myself, in somewhat riper years, that this asperity towards one far advanced in age, and so much my senior, who, with all his extravagancies, always proceeded in good faith, was perhaps not in place; and I hereby gladly retract, not the substance of my words, but the too great acrimony of their form.

The very first Part of Gruithuisen's *Analekten für Erd- und Himmelskunde* (Munich, 1828, 80 pages 8vo) opens with a truly original and remarkable paper of 45 pages, by the editor, "On the Proposal *to dig a hole through the earth*; whether the condition of the air at great depths can be ascertained in any other way, by the excavation of a tunnel transversely through a mountain-range or an arm of the sea, by the *catachthonic observatory*, its mathematical and optical instruments, and also by the elkysmometer." (The words in italics are so in the original.)

At a time when (of course mostly through the fault of Gruithuisen himself and those like him) all so-called physical investigation of the heavens had fallen into such discredit that, in consequence of a natural reaction, professional astronomers rigorously insisted on acknowledging as the substance of Astronomy merely places and times, therefore merely motions and their variations—that (to mention only one instance) even the discovery of the dark ring of Saturn by Galle at Berlin (in the presence of Mädler!) and Vico at Rome, in 1838, could be suppressed or at least remain unnoticed (incredible but true)—at such a time Gruithuisen's memoir could not but seem preposterous; at the present time, when celestial physics are developing so promisingly, much therein contained

will be read only with surprise. I can here intimate only that Gruithuisen proposes, partly, to sink perpendicular shafts of several thousand feet depth, and partly to drive horizontal galleries in the direction of a chord (under the Alps! to a length of 15 miles) through the ground, and, together with practical locomotive purposes, to apply them to physical-astronomical investigations. *L. c.* p. 21, we read, "But it is moreover *incalculable what the astronomer in such a tunnel, provided with a shaft as dry as possible, for observations deserving attention, could accomplish.* Common people would set up a terrible roar of laughter, if told that, under the mountains, at such depths an excellent observatory could be built, in which to make observations of a peculiar sort, which would furnish us with data in the highest degree desirable and to be expected, and a quantity of useful data at present unknown, which must supply to both practical and theoretical astronomy new aids for still greater geometrical accuracy and a great number of new results. To this subterranean observatory I will give the name of *catachthonic observatory* or *catachthonium.*" Now I believe that at the time when the above was printed others besides merely common people would have set up a terrible roar of laughter, if they had seen Gruithuisen's dissertation.

The chief instruments of the catachthonium were, according to Gruithuisen, of two kinds:—first, large, accurately turned rings at the mouths of the shafts, in order on them as on ring micrometers, from a distance of from 100 to 2000 feet, to observe the transits of the stars by day also (in consequence of the presumed visibility of the stars from deep pits), and (p. 22) "to obtain *immediately clear geocentric observations perfectly free from refraction.*" On this it says, at p. 28, "What even these few instruments could accomplish and settle in relation to *the proper motions of many fixed stars, the solstice, precession, nutation, aberration, the course of the moon, and the like*, may be expected to be so much the more, *as the position of the most simple instruments must be the most invariable possible, because here the variations of the temperature are almost* $=0$; *so that the necessary clocks do not require even a compensation-pendulum*; and, besides, there is absolutely nothing present which could be subject to so much as a slight variation of temperature; *wherefore the place of such an observatory cannot be supplied by any observatory above ground, however efficient.*" Here we have Lamont and Carrington's subterranean observatories anticipated a generation previously.

The second principal instrument of the catachthonium was to consist of fine plummets suspended on wires from 150 to 1500 feet in length, in order thereon (according to p. 32) to make "observable *the motion of the earth in its orbit*," "and perhaps even the difference in the annual velocity of this motion." Namely, at pp. 30 and 31 it is shown that, during each rotation of the earth, the rotational velocity of a point in the equator is once added to the orbital velocity, and once subtracted from it, and thereby are produced variations in the horizontal component of the earth's gravitation, which depend

on the double difference of the two velocities. With a plumbline of only 10 feet length, Gruithuisen made some observations which (*l. c.* p. 33) had been related in his work entitled "Lieblingsobjecte auf dem Felde der Naturforschung" (Munich, 1817), pp. 69 *et seq.*, 26, 77, 128. I have not been able to procure a sight of this work. In the *Analekten*, *l. c.* pp. 33, 34, it says of these observations:—"In my very first experiments it proved that this instrument (which I named *elkysmometer*) exhibits effects *which depend not on accidental causes, but on the actions of the gravity and motion of the earth and the increasing nearness of other large heavenly bodies*, which latter would announce themselves through the *plummets* so positively and distinctly, even if we had no ebb and flood. The eastern deviation of the elkysmometer-thread was most striking from 8 to 9 o'clock in the morning . . . . . It was also unmistakable that the moon exerted an attraction on the elkysmometer, especially in the morning, when she was just between the sun and the earth." Also the elkysmometer indicated to him "earthquakes, even from other parts of the world" (*l. c.* p. 34); and on p. 37 the superiority of long plumblines to short ones is demonstrated in detail; indeed a table is given at the end for the reduction of very small sines to arcs for the sake of observations on very long elkysmometers.

With the rough arrangements described, there is no doubt that Gruithuisen's results depended on accidental external disturbances, and perhaps in part on illusion, as a short calculation is sufficient to show that, here, plummets would scarcely ever lead to any result. Only the name proposed by him (ἕλκυσμα = traction, ἕλκω = I draw), though it is not correctly formed and should have been "helkometer," deserves to be adopted.

But the most remarkable thing comes last. In his *Neuen Analekten für Erd- und Himmelskunde*, Band i. Heft 1, published at Munich in 1832 (finished, according to p. 72, "am 27 Juli 1832"), pp. 39 & 40, there is a memoir entitled, "Ritter Bessel's Experiments on the Force with which the Earth attracts bodies of various natures, and of the Editor's Elkysmometer and Hengeller's Swing Balance."

After a report, occupying only 19 lines, on Bessel's pendulum-experiments with gold, silver, lead, iron, zinc, brass, marble, clay, quartz, water, meteoric iron, and meteoric stone, all of which gave, to less than $\frac{1}{60000}$, the same length for the simple seconds-pendulum, it says verbatim*:—

"By these experiments one of my most anxious wishes is fulfilled. Already twenty years since (in 1812), I suspended on wires of several fathoms length bodies of various natures, in order to try whether the opposite positions of the moon towards them would effect any deviation from the vertical line. But as the place was not faultless for such experiments, I held that the results of the experiments were not worth publishing. A pendulum of such a length I called an elkysmometer when a scale to be read with a

* The figure is a faithful fac-simile of the original.

telescope was attached to the wire beneath. I thought to have perceived thereon the daily alternation of forward and backward motion of the earth's surface in relation to the earth's motion in its orbit, and so also with all certainty very distant earthquakes, &c. I wish that some one may find an opportunity to make these observations in a pit, with due accuracy. Reichenbach has proposed for it a very large level [compare A. Wagner's observations of earthquakes in South Europe on the level of the large transit-instrument at Pulkowa]; and I believe that the swing balance made by one of my pupils (of the name of Engeller), adapted on a large scale might, as a preliminary arrangement do excellent service:—

"It consists of a horizontal lever *a b* of brass, on which is fixed at one end a brass ball *c* as a weight; *d* is a fine wire, by which the lever is suspended; instead of the counterpoise, the other arm of the lever is fastened to the floor by the wire *e*; and the instrument becomes the more delicate the nearer the wire *d* comes to the wire *e*. The ball *c* can oscillate only horizontally, and is visibly (according to Hengeller's experiments) attracted by a cannon-ball. It would be very meritorious to institute observations on this instrument.—G."

This is therefore Zöllner's horizontal pendulum complete, scale-reading by telescope and all, though perhaps not with reading-off by mirror; and after the above statements there is certainly no doubt that M. Zöllner's bold idea, of demonstrating the variations of the gravity of the earth and of cosmic attractions by terrestrial observations at one and the same place, had already in 1817, therefore full 52 years previously, been expressed and proved experimentally by Gruithuisen in Munich—and further that the horizontal pendulum, proposed for this purpose by Zöllner, had been constructed and put to the test of experiment, at the latest, in 1832, thus at least 37 years before Zöllner, by a Munich student, a pupil of Gruithuisen, of the name of Hengeller, although unfortunately nothing further about the observations is communicated than that they proved the usefulness of the instrument for the end proposed. Thus, then, this important thought, like so many similar, did not come to light at once and complete, but emerged in a less perfect form a long time previously in isolated original minds, and, because the time was not ripe for it, passed away unnoticed. Hengeller must have been a genius; and since he studied between 1828 and 1832 at Munich, where Gruithuisen was Professor, it might perhaps still be possible to learn something more about his personality. —*Sitzung der math.-naturw. Classe der k. böhmischen Gesellsch. d. Wissenschaften*, November 15, 1872.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

DECEMBER 1873.

LIV. *On Diffraction-Spectrum Photography. By* Henry Draper, *M.D., Professor of Analytical Chemistry and Physiology in the University of New York*.*

[Illustrated by a Photograph (Plate IV.) printed by the Alberttype process.]

THERE are, as is well known, two methods by which spectra may be obtained :—1st, by the action of a prism ; 2nd, by a system of closely ruled lines. In the latter case it is convenient to speak of the contrivance employed as a grating, and of the spectrum as an interference- or diffraction-spectrum. A casual inspection shows that there is a great difference between the spectra produced by these two methods ; and close investigation proves that the diffraction-spectrum is by far the more suitable for accurate scientific work. For this reason it has seemed desirable to make a trustworthy map of those parts of the diffraction-spectrum which can be photographed on collodion, and to attach to it a scale for reading the wave-lengths of the rays.

The Plate (Pl. IV.) accompanying this memoir is from collodion photographs made by myself, transferred to a thick plate of glass, the latter process being known as the Alberttype. For the entire success of this transfer I am indebted to my friend Mr. E. Bierstadt, the owner of the patent in America. The glass is then used in a printing-press in the same manner as a lithographic stone. The picture is absolutely unretouched. It represents, therefore, the work of the sun itself, and is not a drawing either made or corrected by hand.

It consists of two portions—first, the upper, which gives all

* Communicated by the Author, to whom we are also much indebted for the impressions of the beautiful photograph.

the lines of the spectrum from near G to O, or from wave-length 4350 ten-millionths of a millimetre to 3440. Above that is placed a scale which is a copy of Ångström's, from just below G to H_2, with the same-sized divisions carried out from H_2 to O. The second, or lower, is a magnified portion of the same negative, having H_1 and H_2 about its middle, and extending from wave-length 4205 to 3736.

It follows, therefore, that the lines in the solar spectrum are correctly represented in their relative positions. The only errors are those which may have arisen from mal-adjustment of the scale. The precautions that were taken to avoid such errors will be described. With a certain correction, to be mentioned hereafter, it may also be stated that the relative shadings and intensities are preserved.

The value of such a map depends on the fact that it not only represents parts of the spectrum which are with difficulty perceived by the eye (though they may be seen by the methods of Stokes and Sekulic), but also that even in the visible parts there is obtained a far more correct delineation in those portions which can be photographed. In the finest maps drawn by hand, such as those in the celebrated "Spectre Normal du Soleil" of Ångström, the relative intensity and shading of the lines can be but partially represented by the artist, and a most laborious and painstaking series of observations and calculations on the part of the physicist is necessary to secure approximately correct positions of the multitudes of Fraunhofer lines. Between wave-lengths 3925 and 4205 Ångström shows 118 lines, while my original negative has at least 293.

For such reasons many attempts have been made to procure good photographs of the diffraction-spectrum. The earliest were by my father, J.W. Draper; his results were printed in 1843–44 in a work entitled "On the Forces which produce the Organization of Plants." This memoir was accompanied by plates drawn from his daguerreotypes; and the wave-lengths, which he first suggested as the proper indices for designating the Fraunhofer lines, were used as a scale.

Since that time the most important experiments in this direction have been by Mascart and Cornu. These eminent physicists, however, have resorted to the plan of taking portions of the spectrum on a small scale, and subsequently making enlarged drawings therefrom. This course introduces the defects of hand work, and the artistic difficulties of copying intensity and shading, as well as the omission of fine lines.

In the photographs of the spectrum which I have taken, I have tried to get as large a portion as I could at once, and on as large a scale as possible. I have usually obtained images

from below G (wave-length 4307) to above O (wave-length 3440), of about 12 inches (·305 metre long). I have succeeded, however, in photographing from near *b* (wave-length 5167) to T (wave-length 3032), by resorting to a ruled speculum plane and a concave speculum mirror; but the photographic and optical difficulties in securing an enlarged spectrum of that length are great*.

Of course in such a research as this an essential is a finely and evenly ruled plane of glass or other material. Those which I have used were made by a machine devised and constructed by Mr. L. M. Rutherford, whose beautiful lunar and prismatic spectrum-photographs are so well known to the scientific world. The plate generally employed is of glass ruled with 6481 lines to the inch; the ruled part is $1\frac{8}{100}$ inch (·027 metre) long and $\frac{64}{100}$ (·016 metre) wide. It is unquestionably much more nearly perfect than similar gratings made by Nobert and others; for the character of the photographs and the uniformity of the orders on either side of the normal, together with its behaviour under a searching examination, show that it leaves little to be desired. As it is on glass and gives a bright transmitted spectrum, I have constructed the remainder of the optical apparatus of glass achromatized according to the plan used by J. W. Draper in 1843, except that I have not silvered the ruling, and therefore have used the refracted and not the reflected beam. The slit is $\frac{8}{10}$ of an inch (·02 metre) long and $\frac{1}{110}$ of an inch (·00023 metre) wide; the jaws are of steel; and there is not only a micrometer-screw for separating them, but also one for setting them at an angle. Occasionally I have taken photographs with the jaws $\frac{1}{90}$ of an inch (·00028 metre) apart at the top, and $\frac{1}{130}$ (·00019 metre) at the bottom, so as to obtain different intensity in the two edges of the spectrum.

Most of the photographs have been of the spectrum of the third order, which has certain conspicuous advantages. In the first place it is dilated to such an extent as to give a long image and yet one not too faint to be copied by a reasonable exposure of the sensitive plate; and in the second place the spectrum of the second order overlaps it in such a way that D falls nearly upon H, and *b* upon O. These coincidences are serviceable in determining the true wave-lengths of all the rays.

The only point of special interest in connexion with the pho-

* Since writing the above I have succeeded in photographing the lines of the visible spectrum from *b* downward; and the picture comprises not only the regions including E, D, C, B, *a*, and A, but also the ultra-red rays. The great groups α, β, γ below A, discovered by my father in 1843, are distinctly reproduced.

tographic part of the operation is the device for avoiding the unequal action on the sensitive plate of different rays of the spectrum. It has been commonly supposed until the recent memoirs of J. W. Draper, that there are in the spectrum three different types of force in three different but overlapping regions. Heat was supposed to be principally found at the less refrangible end, light in the middle, and actinism at the more refrangible. But he showed that this error has partly arisen from using prismatic spectra, which condense the red end and dilate the violet, and do not present the rays in the true order of their wave-lengths, and partly from the nature of our ordinary photographic substances. He proved that actinism, or the power of chemical decomposition, does not particularly belong to the violet end of the spectrum, but is found throughout its whole length. But bromide and iodide of silver as used in collodion photography are more easily decomposed by vibrations of certain lengths and periods than by others; and hence the excess of action seen at the violet end is a function of certain silver compounds, and not of the spectrum. Other substances, as carbonic acid, show maxima elsewhere, as in the yellow region. The solar beam is therefore not compounded of three forces, light, heat, and actinism, but it is a series of æthereal vibrations which give rise to one or other of these manifestations of force, depending on the surface upon which it falls.

In order to provide against this excees of action in certain parts of the spectrum, I introduced a system of diaphragms placed in the vicinity of the sensitive plate and removed at suitable times during the exposure. The region from wave-length 4000 to 4350 only requires about one tenth of the time demanded by that from 3440 to 3510. In the negative which produced the accompanying Plate the line O had 15 minutes, and G $2\frac{1}{2}$ minutes; and the former is underexposed. These exposures seem at first sight unusually long for a wet collodion surface; but it must be remembered that the slit used was only $\frac{1}{110}$ of an inch wide, and that the diffraction-grating gives an almost complete circle of spectra round itself, amongst which this thin band of light is divided. A beam $\frac{1}{110}$ of an inch (·00023 metre) wide is spread out in this case into a streak about 78 feet (23·77 metres) long.

After the production of spectra that were in focus from end to end, it was next necessary to attach a scale to them by which the wave-lengths might be read. At first I tried by reducing Ångström's maps to the proper dimensions to accomplish this object; but the undertaking proved to be difficult, and was unsuccessful, because, though the original drawing on the stone was undoubtedly correct, the paper proof of it, which I had, had

stretched unequally in printing; and on applying a photographic reduction to my spectra, coincidence could not be obtained. As, however, the subject of providing a scale for these diffraction-spectra is of prime importance in giving value and precision to the wave-lengths presented in this memoir, I propose to describe fully the method eventually employed in fitting a scale to the photograph.

The wave-lengths of the ultra-violet rays have never, so far as I know, been either determined or published except by J. W. Draper in 1844, Mascart in 1866, and Cornu in 1872. J. W. Draper's memoir has a steel engraving of some of the principal lines, from which the wave-lengths may be approximately read.

The large plate which accompanies Mascart's long and valuable memoir is of the prismatic spectrum; but he furnishes in addition the following Table of wave-lengths:—

L	3819·0
M	3728·8
N	3580·2
O	3440·1
P	3360·2
Q	3285·6
R	3177·5

The numbers do not entirely coincide in all cases with my photograph, as I will show further on.

The detailed results of M. Cornu have not appeared in any publication that has reached me.

I have used as a basis the numbers given by Ångström for the rays D_2, b_4, and G; and if there should be any small error in his determinations, my scale will require a proportionate correction, which can easily be effected. At first it seemed better to take G and H as fixed points; but the line H is so broad, and has so many component lines, that its position is uncertain, and, moreover, being almost at the limit of visibility in Ångström's apparatus, it was more open to errors of measurement. These reasons led me to take advantage of the fact that the second spectrum overlaps the third, the ray D of the second being near H of the third, and *b* of the second near O of the third. It is obvious that we have thus the means of ascertaining the wave-lengths of three parts—one at each end, and one in the middle of my photograph. As the rays D and *b* cannot impress themselves on collodion by any length of exposure that it is convenient to give, and as in my method of working the ultra-violet rays could not be seen simultaneously with them, it was necessary to resort to the following device. I placed in front of the sensitive plate and close to it two fine steel points, one of which

was carefully adjusted to the position of D_2 of the second order, and the other to b_4 of the second order. When, therefore, after a suitable exposure to the ultra-violet spectrum of the third order the collodion picture was developed, there were two sharply defined images of the steel points superposed on the spectrum. The point which had been coincident with D_2 of the second order was then found to have cast its shadow on H_2 of the third order; and the point at b_4 of the second order had impressed itself near O of the third order.

By a simple calculation it was thus rendered evident that a given ray in the compound line H_2 was of the wave-length 3930·1 ten-millionths of a millimetre, and that another line near O had the wave-length 3444·6. By looking at the photograph the reader will see that 3930 falls upon a fine division in H_2, which is beautifully shown in both the spectrum with the scale and the enlarged proof below. Of course the ray G of the third order, the wave-length of which is known, had impressed itself photographically on the collodion.

Having thus ascertained the wave-lengths of three fixed points in the photograph, the next step was to apply a scale reading to a single ten-millionth of a millimetre, and, if possible, fractions thereof. After many abortive attempts to use that part of Ångström's map which lies between G and H, and to attach thereto an additional length of scale sufficient to extend to the end of the ultra-violet region, I was compelled to resort to a linear dividing-engine, and rule a scale which was about twice the length of the photographic reduction shown in the accompanying Plate. Of course this necessitated drawing in by hand the same systems of lines and lettering as are shown on Ångström's chart; and this I did as carefully and faithfully as I could.

It only remained to reduce this divided scale to the proper size to fit the spectrum-photograph; after many trials it was accomplished.

It is proper in this place to make a criticism on my scale and to point out a small error, which may be due, however, to an incorrect determination of the wave-lengths that I have used as fixed points. Taking the distance from G (wave-length 4307) in the photograph to the fixed line 3930 in H_2 and dividing it into 377 parts, and then prolonging these divisions towards O, it was found that the third fixed point was not attained, but that there was an error of about two divisions. But if the position of D_2 in Ångström's determinations be incorrect to the extent of one ten-millionth of a millimetre, or if this small error should be partly attributed to D_2 and partly to G, my scale will be correct. Future measures of the wave-lengths of these rays and of b_4 can alone settle this delicate point; for the deter-

minations of Mascart and Ångström and Thalen differ nearly to the extent mentioned above. The same remark is true of Ångström compared with Ditscheiner, while the difference between Ångström and Van der Willigen is more than three times the amount necessary to remove my discrepancy. In any case the photograph is correct, as it is the work of the sun, and is only open to errors arising from imperfect flatness in the field of a fine lens, and that field only subtending an angle of about 4°. The angular aperture of the lens viewed from the sensitive plate is 20′. I trust, therefore, that the photograph may be of permanent value to physicists; for any one can affix another scale if this be erroneous.

An examination of the photographed spectrum shows many points of interest, some of which are best seen in the spectrum with the scale above, and some in the portion enlarged below. The latter is magnified about twice, and comprises the region from wave-length 3736 to 4205. I have also made photographs on the same scale as Ångström's map, but have not as yet printed them. The capital letters which are attached to the region above H are according to the nomenclature of Mascart, although the wave-lengths assigned by him to those letters do not coincide exactly in all cases with the lines in my photograph; for instance, the line L, which he regards as single, is in reality triple, and does not correspond to 3819 but to 3821; M is correctly designated by 3728, but it is double; N is really at 3583 and not at 3580. It has been suggested that it would be proper to return to the old nomenclature of Becquerel and J. W. Draper, who simultaneously discovered these lines in 1842–43; but the designation of position by wave-length in reality renders the letters unnecessary.

The spectrum above H, when compared with the region from G to H, is marked by the presence of bolder groups of lines; and most conspicuous are those between 3820–3860, 3705–3760, 3620–3650, 3568–3590, 3490–3530. The first of these groups is strikingly shown in the enlarged photograph. I am not as yet able to offer an opinion as to the chemical elements producing these groups; for almost all the photographs of the ultraviolet spectra of metalline vapours that I have thus far made were produced by a quartz train, and have not yet been reduced to wave-lengths; indeed that is a separate field of inquiry, and could not be comprised in a memoir of this length. I have also tried to utilize the photographic spectra of the late Professor W. A. Miller, published in the Transactions of the Royal Society for 1862; but, for some reason (probably insufficient intensity of the condensed induction-spark), his pictures do not bring out the peculiarities of the various metals in the striking manner

that is both necessary and attainable. The diffraction-spectra of metalline vapours that I have made are not yet ready for use.

The probability is that each of these groups will be found to be due to several elements, as is plainly seen in the group H. This compound line, which is commonly spoken of as being caused by calcium, iron, and aluminium, is in reality much more complex; for there can readily be counted in it more than fifty lines in the original negative; and a careful inspection of the accompanying paper picture shows a large proportion of them. This observation leads us to a more general statement. *The exact composition of even a part of the spectrum of a metal will not be known until we have obtained photographs of it on a large scale.* The coincidences which were so thoroughly examined by Mr. Huggins (Trans. Roy. Soc. Dec. 1863) will only disappear when we can, in addition to the position of a line, have a clear idea of its size, strength, and degree of sharpness or nebulosity. The eye is not able to see all the fine lines; or even if it does, the observer cannot map them with precision, nor in their relative strength and breadth. For example, in Ångström's justly celebrated chart, of which the G–H portion is copied in this Plate; and in the construction of which the greatest pains were taken by him, many regions are defective to a certain extent. The region from 4101 to 4118 is without lines; yet the photograph shows in the enlarged copy seventeen that can easily be counted, and the original negative shows more yet. The reader, of course, understands that a paper print of a collodion picture is never as good as the original, the coarseness of grain in the paper, want of contact in transferring, &c. effect such a result. Moreover the Alberttype process depends on a certain fine granulation which is given to the bichromated gelatine; and this forbids the use of a magnifier upon these paper proofs. It is only just, however, to Mr. Bierstadt to state that, without his personal supervision, such sharp and fine-grained proofs could not have been obtained, and that no other printing-press process that I know of could have accomplished this work at all. As an illustration of the difficulty of depicting the relative intensity of lines, we may examine 3998, which in Ångström's chart is shown of equal intensity with 4004, while in reality it is much fainter, and instead of being single is triple, as is well shown in the enlarged spectrum.

When, however, we compare Ångström's chart with the photograph, it requires, as the above remarks show, a critical examination to detect defects, and we have a striking confirmation of the surprising accuracy of the Swedish philosopher.

So also in comparing Mascart's excellent map of the prismatic spectrum with the photograph, the difficulty of depicting all the

fine lines is seen. In the group L he shows 12 lines, while even in the Alberttype copy of my photograph 25 can be counted, and in the original negative many more. From H to L he shows 70 lines; in my Plate 138 can be observed besides many unresolved bands.

In the earlier part of this memoir it was stated that the relative intensities of the lines in the spectrum were correctly represented if a certain allowance was made. If an unshielded collodion plate were presented to the image of the spectrum, there would be produced a stain very dense from G to H, fainter above H, and still fainter below G. But this stain would not represent the actinic force of the sun, it would merely be an index of the decomposability of a mixture of iodide and bromide of silver. I have for this reason adopted the idea of J. W. Draper, that force is equally distributed through the spectrum, and have tried to produce a photograph of equal intensity throughout. This has been accomplished, as I have before stated, by suitable diaphragms. But whether this view be correct or not, lines which are not far distant from one another are presented virtually without any interference by diaphragms, and must therefore be correct both as to shading and intensity.

Besides the points above mentioned, there are many theoretical considerations suggested by the photograph which it does not seem expedient to enter upon fully at present. Among such is the possibility of arriving at an estimate of the sun's temperature by interpreting the apparent bands, such as those near G and H, by the aid of Lockyer's researches on the temperature of dissociation of compounds. No one has yet ascertained whether there are or are not unresolvable bands in the solar spectrum. If they do exist, the compounds to which they belong, and the necessary temperature for dissociation, remain to be determined.

It would seem also to be possible to find out whether, as asserted by Zöllner, there is a liquid envelope round the sun, by a search for more diffused bands in its photographed spectrum.

In the hope that this photograph may prove to be of value to scientific men for further investigations upon the sun and the elements, I have caused a number of extra copies to be printed, and shall be glad to present them to any one who can make use of them.

University, Washington Square,
New York.

LV. *On* Hamilton's *Dynamic Principle in Thermodynamics.* *By* C. Szily*.

In a memoir published not long since†, I have maintained that the equation which expresses the second proposition of the mechanical theory of Heat is in complete agreement with the dynamical equation which is named after Hamilton, and that, accordingly, in reply to the question "To which equation in dynamics can the second proposition of thermodynamics be reduced?" we must point directly to Hamilton's equation.

I was moved to the publication of my considerations on this subject chiefly by the circumstance that neither Boltzmann nor Clausius, in their memoirs‡, has once mentioned Hamilton's principle; and they are the only writers who up to that time had occupied themselves with the above question. Both seem to consider that, in order to bring in accordance, it was necessary to deduce an equation yet unknown in dynamics; both remark that the equations deduced by them exhibit a certain affinity with the principle of least action; but neither of these two philosophers has taken any notice of Hamilton's principle and its close relations to the equations in question.

Hereupon M. Clausius published a memoir§ which refers exclusively to mine above mentioned; and at the very commencement he therein readily acknowledges that the connexion sought becomes much more striking when one compares the proposition in question, not (as he and Boltzmann had done) with the principle of least action, but with Hamilton's. At the same time Clausius suggests the cause which prevented him from turning his attention to Hamilton's principle when treating upon the above-mentioned connexion. Namely, in many text-books of dynamics (for instance, Jacobi's *Vorlesungen über Dynamik*) Hamilton's equation is cited in a form which differs essentially from the original, in consequence of the peculiar variation. Now, if Jacobi's form only be kept in view instead of the primitive form,

* Translated from a separate impression, communicated by the Author, from Poggendorff's *Annalen*, vol. cxlix. pp. 74–86.

† "Hamilton's Principle and the Second Proposition of the Mechanical Theory of Heat," Phil. Mag. S. 4. vol. xliii. p. 339. Pogg. *Ann.* vol. cxlv. p. 295.

‡ Boltzmann, "Ueber die mechanische Bedeutung des zweiten Hauptsatzes der Wärmetheorie," *Sitzungsberichte der Wiener Akademie*, vol. liii. p. 210. R. Clausius, "Ueber die Zurückführung des zweiten Hauptsatzes der mechanischen Wärmetheorie auf allgemeine mechanische Principien," Pogg. *Ann.* vol. cxlii. p. 433; Phil. Mag. S. 4. vol. xlii. p. 161.

§ Pogg. *Ann.* vol. cxlvi. p. 585. "On the Connexion of the Second Proposition of the Mechanical Theory of Heat with Hamilton's Principle," Phil. Mag. S. 4. vol. xliv. p. 365.

it is indeed true that it is not very conceivable how any connexion capable of being clearly indicated can exist between that equation and the second proposition of the mechanical theory of heat.

Clausius then quotes the original form of Hamilton's equation used by me, and proves that Boltzmann's equation (23*a*), rightly interpreted, perfectly corresponds with Hamilton's, is indeed identical with it. But what he concedes in relation to Boltzmann's equation, that he decidedly questions in reference to his own; nay, he declares without hesitation the correspondence impossible. Thus the same question is again raised which has been discussed in the *Annalen,* between Clausius and Boltzmann*, on the occasion of the claiming of priority by the latter, namely:—Does there actually exist a difference, and, if this is the case, what is the difference, between the equation of Clausius and that of Boltzmann? I believe that I am now entitled—nay, after the reply of Clausius, am bound to enter into the discussion of the question, at least in regard to Hamilton's equation.

I will first discuss the equation which Clausius claims as his own.

For the more convenient elucidation of the matter, we will first of all, as Clausius does, confine ourselves to the consideration of the periodic motion of a single point. Given, therefore, a material point which with the original motion describes a closed path, or moves between two given points. The only forces acting on the movable point shall be such as possess a force-function (or, as Clausius is accustomed to express it, an ergal)—that is, forces of which the components can be represented by the partial differential quotients, taken negatively, of a function of the coordinates of the point. Let us now suppose that this motion undergoes an infinitesimal alteration. With the changed motion let the point likewise describe a closed path, or move between two points, which latter may be either identical with those previously given, or, if not so, fulfil the condition that the quantity

$$\frac{dx}{dt}\delta x+\frac{dy}{dt}\delta y+\frac{dz}{dt}\delta z$$

has the same value at the terminal point of the motion as at the initial point. Then let i denote the period of the motion, T the *vis viva,* U the force-function; and let us agree to indicate the mean value of the variables by putting a horizontal stroke over

* Pogg. *Ann.* vol. cxliii. p. 211: "Zur Priorität der Auffindung der Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und dem Principe der kleinsten Wirkung; von L. Boltzmann." Ibid. vol. cxliv. p. 265: "Bemerkungen zu der Prioritätsreclamation des Hrn. Boltzmann; von. R. Clausius."

them (for example, $\bar{x}=\frac{1}{i}\int xdt$): then the equation to which Clausius in his answer appeals, and which he marked (18) in his first memoir on this subject*, is the following:—

$$\overline{\frac{dU}{dx}\cdot\delta x+\frac{dU}{dy}\cdot\delta y+\frac{dU}{dz}\cdot\delta z}=\delta\bar{T}+2\bar{T}\,\delta\log i. \quad . \quad . \quad (1)$$

Now this is the equation to which Clausius refers as his own, and which, he quite correctly remarks, has a more general applicability than Hamilton's. It is also indeed impossible to find complete accordance between this and Hamilton's equation. I must, however, observe that I have not reckoned *this* equation as Clausius's and designated it as such; neither can I now so designate it; for this equation was known five years since, as it was given and demonstrated in the first volume of Sir William Thomson's work, so widely diffused and frequently cited amongst physicists, the 'Treatise on Natural Philosophy,' Oxford, 1867 (translated into German by G. Wertheim: Brunswick, 1871). If, namely, the equation (9) in the section "Dynamical Laws and Principles"† be written with Clausius's notation, it exhibits the following form:—

$$\delta(2i\bar{T})=i\left[\overline{\delta T}+\overline{\frac{dU}{dx}\,\delta x+\frac{dU}{dy}\,\delta y+\frac{dU}{dz}\,\delta z}\right]. \quad . \quad . \quad (2)$$

Now it is clear that this equation is identical with equation (1) above cited.

Moreover, for a second and not less important reason I could not, when speaking of the equation of Clausius, mean the above-mentioned equation of Thomson: namely, Thomson's equation is of such generality that, without a special hypothesis, it cannot be brought into consonance with the second proposition of the mechanical theory of heat. For if we could apply this equation in its entire generality to that motion which we call heat, then (as we shall see further on) the second proposition of the mechanical theory of heat would not be true. We can, however, most simply bring Thomson's equation into harmony with the second proposition of thermodynamics, if we make a term vanish from the dynamic equation, putting it $=0$. And because it was

* Pogg. *Ann.* vol. cxlii. p. 442; Phil. Mag. S. 4. vol. xlii. p. 167. This is equation (2) of his memoir "Ueber die Anwendung einer von mir aufgestellten mechanischen Gleichung" &c., *Nachr. der kgl. Ges. d. Wiss. zu Göttingen*, 1871, p. 248; *Math. Ann von* Clebsch *u.* Neumann, vol. iv. p. 232; Phil. Mag. S. 4. vol. xlii. p. 321.

† In order fully to set forth the importance of this equation, Thomson adds:—"This, it may be observed, is a perfectly general kinematical expression, unrestricted by any terminal or kinetic conditions."

Clausius who showed which term must be omitted from that equation, and under what assumption this can be attained, I think that the complete truth of the historical development is attained when we, as I have done, designate as Clausius's the special equation* resulting from the assumption above alluded to, viz.

$$\overline{\delta \mathrm{U}}=\overline{\delta \mathrm{T}}+2\overline{\mathrm{T}}\delta \log i. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

Now, what is the difference between this and Hamilton's equation?

Before entering further into the clearing-up of this question, let us inquire generally what kind of conditions must be fulfilled in order to bring Thomson's equation into agreement with the second proposition of the mechanical theory of heat. For the sake of a more convenient survey, I will write Thomson's equation in a somewhat different form. If E denote the total energy, therefore the sum of $\overline{\mathrm{T}}$ and $\overline{\mathrm{U}}$, we can, after a slight transformation, write equation (2) as follows:—

$$\delta \mathrm{E}=\frac{\delta(2i\overline{\mathrm{T}})}{i}+\overline{\delta \mathrm{U}}-\overline{\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\delta z}, \quad . \quad (4)$$

or, if we restore the definite integral in the last term, from which the mean value denoted by the horizontal stroke has arisen, we obtain

$$\delta \mathrm{E}=\frac{\delta(2i\overline{\mathrm{T}})}{i}+\frac{1}{i}\int_0^i\left[\delta \mathrm{U}-\left(\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\delta z\right)\right]dt. \ (5)$$

If, finally, we divide the entire equation by $\overline{\mathrm{T}}$, it becomes

$$\frac{\delta \mathrm{E}}{\overline{\mathrm{T}}}=\delta 2\log(i\overline{\mathrm{T}})+\frac{1}{i\overline{\mathrm{T}}}\int_0^i\left[\delta \mathrm{U}-\left(\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\right)\delta z\right]dt.$$

It is plain that this equation, which is still identical with Thomson's, can only be brought into the form of the second proposition when it is possible to put the last term equal to the total variation of any invariable function of the coordinates—that is, when

$$\int_0^i\left[\delta \mathrm{U}-\left(\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\delta z\right)\right]dt=i\overline{\mathrm{T}}\,\delta \mathrm{F}. \quad . \quad (6)$$

Supposing this value introduced into the last equation, and the symbols of the differentials substituted for those of the variations, then

$$\frac{d\mathrm{E}}{\overline{\mathrm{T}}}=d2\log(i\overline{\mathrm{T}})+d\mathrm{F}.$$

* This is Clausius's equation (21) in Pogg. *Ann.* vol. cxlii. p. 442; Phil. Mag. S. 4. vol. xlii. p. 170.

If we integrate this equation throughout an entire cyclical process, taking at the same time into consideration that $i\overline{\mathrm{T}}$ as well as F (according to the assumption) has at the beginning and at the end of the process the same value, then is

$$\int \frac{d\mathrm{E}}{\mathrm{T}} = 0.$$

Thomson's equation is therefore only then in complete accordance with the second proposition of the theory of heat, when the equation of condition (equation 6) is satisfied.

Let us now see in what manner this condition is fulfilled, on the one hand, in Hamilton's, and, on the other, in Clausius's equation.

Hamilton founded his equation on the assumption* that the form of the force-function does not change with the change of the motion, that hence $\delta\mathrm{U}$ depends solely on the variation of the space-coordinates—that is, that

$$\delta\mathrm{U} - \left(\frac{d\mathrm{U}}{dx}\delta x + \frac{d\mathrm{U}}{dy}\delta y + \frac{d\mathrm{U}}{dz}\delta z\right) = 0. \quad . \quad . \quad (7)$$

In consequence of this,

$$\delta\mathrm{F} = 0,$$

and equation (5) takes the following form:—

$$\delta\mathrm{E} = \frac{\delta(2i\overline{\mathrm{T}})}{i}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

This (8) is Hamilton's equation.

Now, in what consists the assumption made by Clausius? He admits that even the form of the force-function may change; he only lays down this condition†, that the variation which results from the change of form of the force-function, taken at its mean value, is equal to 0, and therefore

$$\overline{\delta\mathrm{U}} - \overline{\frac{d\mathrm{U}}{dx}\delta x + \frac{d\mathrm{U}}{dy}\delta y + \frac{d\mathrm{U}}{dz}\delta z} = 0,$$

that is,

$$\int_0^i \left[\delta\mathrm{U} - \left(\frac{d\mathrm{U}}{dx}\delta x + \frac{d\mathrm{U}}{dy}\delta y + \frac{d\mathrm{U}}{dz}\delta z\right)\right] dt = 0. \quad . \quad (9)$$

Therefore according to this assumption also

$$\delta\mathrm{F} = 0.$$

Consequently equation (1) takes the form of equation (3), viz.

$$\delta\overline{\mathrm{U}} = \delta\overline{\mathrm{T}} + 2\overline{\mathrm{T}}\delta \log i.$$

* Phil. Trans. 1834, p. 249, *sub* (1).

† Pogg. *Ann.* vol. cxlii. pp. 442–446, *sub* (18)–(22); Phil. Mag. S. 4. vol. xlii. pp. 167–170.

This is Clausius's equation.

Now I affirmed that *this* equation, and not Thomson's, is identical with Hamilton's. This assertion of mine I still maintain in its full extent; and I believe that after the preceding statements Professor Clausius will urge no objection to it.

Still, although Clausius's mathematical contemplations on this subject have not led to an altogether new equation, but have for their result merely Thomson's and Hamilton's equations somewhat transformed, it would be a great mistake to suppose that those highly important disquisitions had remained unfruitful. As Boltzmann on his part, so I also most willingly acknowledge that the portion which chiefly refers to the change of form of the force-function was absolutely necessary—nay, that only with their aid could Hamilton's equation have been, with any result, introduced into the mechanical theory of heat. Hamilton has proved his equation only for the case in which the form of the force-function undergoes no change when the motion is changed; but from the researches of Clausius it follows that *the same* equation (Hamilton's) can be made use of also when the form of the force-function changes, provided that the mean value of the resulting variation be $=0$. Such an extension of the validity of Hamilton's equation was at all events requisite, because in the thermodynamic theory changes of state of bodies come into consideration which are not dependent merely on space-alterations, but also on the changed form of the force-function.

Let us now examine how far Hamilton's equation must be modified to make it applicable to the case in which we have to do not merely with a single point, but with the change of motion of an entire system of points. And indeed let us just take a system the individual points of which do not travel their paths with equal *vires vivæ* or in equal times. Professor Clausius remarks, quite correctly, that in this case Hamilton's equation is not immediately applicable, and consequently special investigations were necessary.

When, however, we consider the matter a little more closely, we find that in the form (8) of Hamilton's equation no other modification is required than that which, relative to the energy, naturally results from the notion of a heterogeneous system of points; that is, we have only to prefix the sign of summation to the right- and left-hand terms, and to extend this summation to all the points of the system. Thus, whilst Hamilton's equation expresses the variation of the energy *for one point* as follows,

$$\delta E = \frac{\delta(2i\overline{T})}{i},$$

we obtain the variation of the energy relative to an *entire system*

of points in a perfectly analogous form by the equation

$$\Sigma\delta E=\Sigma\frac{\delta(2i\bar{T})}{i}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

But this modification is truly so slight, and follows so naturally from the notion of the system, that we may regard equation (10) as the necessary consequence of Hamilton's equation. As, however, this equation does not occur in Hamilton, and he limited the employment of the summation-symbol to the case of the points all accomplishing their paths in the same time, it will perhaps not be unadvisable to effect a special demonstration, after Hamilton's method, of equation (10). It is moreover to be remarked that this equation is first found in Boltzmann, and that the demonstration here given deviates essentially from his only at the commencement, and afterwards only with respect to the arrangement.

Let m be the mass of any point whatever of a system, x, y, z its rectangular coordinates, v the velocity, T the *vis viva*, U the force-function, E the energy, and i the period of a revolution. T and U have different values at different places in the path; but their sum (that is, E) will remain constant so long as the point describes the same path. Hence this quantity is independent of the time. Let the motion of the point undergo a change: instead of its former closed path and periodic motion, let it follow a new closed path infinitesimally different from the previous one. In this new path the mass of the point alone remains as before; all the rest of the quantities are in general changed. Let the variations springing from the change of path be denoted by δ. It follows, from the conception of the energy, that

$$\delta E=\delta T+\delta U.$$

If we multiply this equation by the time-element and integrate it from 0 to i, and at the same time take into account that both E and δE are independent of the time, then

$$\delta E=\frac{1}{i}\int_0^i(\delta T+\delta U)dt.$$

Let us form analogous expressions for the other points of the system, and sum them:

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i(\delta T+\delta U)dt,$$

which can also be written thus:—

$$\Sigma\delta E=\Sigma\frac{1}{i}\int_0^i\left(\delta T+\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)dt$$
$$+\Sigma\frac{1}{i}\int_0^i\left[\delta U-\left(\frac{dU}{dx}\delta x+\frac{dU}{dy}\delta y+\frac{dU}{dz}\delta z\right)\right]dt.$$

Supposing now that the last term on the right-hand side (that is, the sum of the mean values of the variations resulting from the change of form of the force-function) is $=0$, there remains

$$\Sigma\delta\mathrm{E}=\Sigma\frac{1}{i}\int_0^i\left(\delta\mathrm{T}+\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\delta z\right)dt.$$

Substituting

$$\delta\mathrm{T}=mv\,\delta v$$

and

$$-\left(\frac{d\mathrm{U}}{dx}\delta x+\frac{d\mathrm{U}}{dy}\delta y+\frac{d\mathrm{U}}{dz}\delta z\right)$$
$$=m\left(\frac{d^2x}{dt^2}\delta x+\frac{d^2y}{dt^2}\delta y+\frac{d^2z}{dt^2}\delta z\right)=m\frac{dv}{dt}\delta s,$$

where δs signifies the variation in the length of the path, we obtain

$$\Sigma\delta\mathrm{E}=\Sigma\frac{1}{i}\int_0^i m\left(v\delta v-\frac{dv}{dt}\delta s\right)\delta t.$$

Now

$$\int_0^i\frac{dv}{dt}\delta s\,.\,dt=v_1\delta s_1-v_0\delta s_0-\int_0^i v\frac{d(\delta s)}{dt}dt,$$

where the indices 0 and 1 in the integrated portions denote the initial and final values of the quantities. But as the path is closed and the motion periodic,

$$\delta s_1=\delta s_0 \text{ and } v_1=v_0.$$

Accordingly

$$\Sigma\delta\mathrm{E}=\Sigma\frac{1}{i}\int_0^i m\left(v\delta v+v\frac{d(\delta s)}{dt}\right)dt.$$

Taking into consideration that

$$vdt=ds,$$

by a slight simplification we get

$$\Sigma\delta\mathrm{E}=\Sigma\frac{1}{i}\int_0^i m\delta(v^2dt).$$

If we bring the constant mass under the variation-symbol and reverse the succession of the integration and variation, we have

$$\Sigma\delta\mathrm{E}=\Sigma\frac{1}{i}\delta\int_0^i 2\mathrm{T}dt,$$

or, designating the mean value by a horizontal stroke,

$$\Sigma\delta\mathrm{E}=\Sigma\frac{\delta(2i\overline{\mathrm{T}})}{i}.$$

Now this is the equation which we wished to demonstrate.

I will remark finally that, when the paths of the points are not closed, the transition from closed paths to those not closed can be effected in precisely the same manner, whether we write the Hamilton-Boltzmann equation in its primitive form or in the logarithmic form as modified by Clausius. In truth the form of the equation does not occasion, nor does it prevent, in this case the difficulties mentioned by Professor Clausius at the end of his memoir.

For an easier view of the question, let us compare the dates of the development of that dynamic equation to which the second proposition of thermodynamics is reducible. Hamilton set up the equation in the year 1834, but presupposed in the demonstration that the system of points is homogeneous, and the form of the force-function invariable. In 1868 Boltzmann applies this equation to a heterogeneous system, and, for the first time, brings it into the well-known form of the second proposition of the mechanical theory of heat. In 1871 Clausius proves that the same equation is also valid when the form of the force-function changes, but the mean value of the resulting variation $=0$. Consequently, within these thirty-eight years, only the validity, the applicability, of the equation has been extended, without the equation itself undergoing any essential alteration, even in form. I repeat the concluding words of my previous memoir:—"What in thermodynamics we call the second proposition, is in dynamics no other than Hamilton's principle, the identical principle which has already found manifold applications in several branches of mathematical physics."

LVI. *On the Fundamental Modes of a Vibrating System.* *By* Lord Rayleigh, *M.A., F.R.S.**

THE motion of a conservative system about a configuration of stable equilibrium may be analyzed into a series of normal component vibrations, each of which is entirely independent of the others. When one of the components exists alone, the motion is simple harmonic, the period of the vibration and the phase at any given moment of time being the same for all parts of the system. In such a case the system is said to vibrate in a fundamental mode. In order to represent the most general kind of motion, the whole series of normal components must be imagined to exist together, the amplitude and phase of each being arbitrary.

The number of the normal components, which also expresses the degree of freedom enjoyed by the system, may be either finite or infinite, though, strictly speaking, every natural system be-

* Communicated by the Author.

longs to the latter class. It is only by the introduction of limitations, such as attributing to various parts absolute rigidity, that the position of the whole is reduced to dependence on a finite number of coordinates. But even after every permissible simplification has been made, there remains a large and important class of systems whose configuration cannot be defined without the specification of an infinite number of coordinates, or, which comes to the same, of a function of one or more independent variables. Under this head must be included all strings, bars, membranes, plates, &c. which are treated as capable of continuous deformation.

To each fundamental mode corresponds what may be called a normal function. To determine these in the case of any particular system is a problem which may tax, and will usually overtax, all the power of analytical expression which the mathematician possesses; but whether expressed in terms of simple functions or not, the normal functions must be thoroughly discussed, not to say tabulated, before the solution of the problem can be considered complete.

The normal functions appear analytically as the solution of a differential equation containing a constant at this stage undetermined; and the first step is the formation of this characteristic equation. The usual method proceeds by the consideration of the forces actually acting on an element in virtue of its connexion with the rest of the system. For example, the element of a flexible string is acted on by the tensions at its two extremities; and the equation of motion expresses the fact that the actual acceleration of the element is proportional to the resultant of the tensions when resolved in the transverse direction. The characteristic equation is obtained on the introduction of the assumption that the whole motion is simple harmonic.

The second method, which was (I believe) first employed by Green, depends on the use of what we now call the potential energy; and my present object is to point out its advantages. For this purpose I will take, as neither too easy nor too difficult, the problem of the transverse vibrations of a thin uniform rod whose natural condition is straight.

The potential energy V is for each element of length proportional to the square of the curvature; and thus, if y denote the transverse displacement of the element whose distance from one end of the rod is x, we have

$$V=\tfrac{1}{2}B\int\left(\frac{d^2y}{dx^2}\right)^2 dx, \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where the integration must extend over the length of the rod, from 0 to l.

If ρ be the longitudinal density, we have by the Principle of Virtual Velocities as the variational equation of motion,

$$\delta V + \int \rho \ddot{y} \delta y = 0 \; ; \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where δy refers to a hypothetical variation of y, which is subject only to the condition of not violating the connexion of the system. In the formation of (2) we have, as is usual, neglected the reaction of the elements of the bar against the acceleration of rotation.

In order to deduce the ordinary form of the differential equation and the terminal conditions, we require to transform the expression δV. By the usual method we find

$$\delta V = B \int_0^l \frac{d^2 y}{dx^2} \frac{d^2 \delta y}{dx^2} dx$$

$$= B \left\{ \frac{d^2 y}{dx^2} \frac{d \delta y}{dx} \right\} - B \left\{ \frac{d^3 y}{dx^3} \delta y \right\} + B \int_0^l \frac{d^4 y}{dx^4} \delta y \, dx \; ;$$

and therefore, as in the Calculus of Variations,

$$B \frac{d^4 y}{dx^4} + \rho \ddot{y} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The integrated terms give us the conditions to be satisfied at the extremities, namely

$$\frac{d^2 y}{dx^2} \delta \frac{dy}{dx} = 0, \quad \frac{d^3 y}{dx^3} \delta y = 0. \quad . \quad . \quad . \quad . \quad . \quad (4)$$

There are three cases to be considered. At a clamped end δy and $\delta \frac{dy}{dx}$ vanish, and the equations (4) are satisfied without any further supposition. At a free end δy and $\delta \frac{dy}{dx}$ are arbitrary, and hence

$$\frac{d^2 y}{dx^2} = 0, \quad \frac{d^3 y}{dx^3} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

are the conditions to be satisfied in such a case. The third case occurs when the end is constrained to be a node, but the direction of the rod is left free. Since $\delta \frac{dy}{dx}$ is arbitrary, the conditions are

$$y = 0, \quad \frac{d^2 y}{dx^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The general equation (3) and the terminal conditions are the same as would be found without the use of the potential energy by the ordinary method.

In order to determine the normal functions, we assume that $y=u\cos pt$, where u is a function of x. We thus obtain

$$\mathrm{B}\frac{d^4u}{dx^4}-\rho p^2u=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

from which u is found as the sum of four distinct functions of x (which it is not necessary to write down), each multiplied by a coefficient, which is up to this point arbitrary. The four equations expressing the terminal conditions determine the *ratios* of the four coefficients, and cannot be harmonized without ascribing special value to p. The condition expressing the compatibility of the four equations limits p to certain definite values which appear as the roots of a transcendental equation. Corresponding to each admissible value of p there is thus a normal function in which every thing is definite except a constant multiplier.

Since the most general motion may be compounded of the normal component vibrations, it follows that the most general value of y may be expressed in the series

$$y=\phi_1u_1+\phi_2u_2+\ldots; \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where u_1, u_2, &c. are the normal functions, and ϕ_1, ϕ_2, &c. arbitrary coefficients. The determination of ϕ_1 &c. is effected by means of the characteristic property of the normal functions, that the product of any two of them vanishes when integrated over the length of the rod. From this it follows that

$$\int_0^l yu_rdx=\phi_r\int_0^l u_r^2dx. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The fact that the normal functions are *conjugate* (which is the name given to functions possessing the property above stated) may be proved from the form of the functions, or better from the differential equation and terminal conditions which define them. The last course is that which has been followed by the distinguished mathematicians who have treated of the present and similar questions.

If u and v be two normal functions corresponding to p and p', we have from (7),

$$\frac{\rho}{\mathrm{B}}(p'^2-p^2)\int_0^l uvdx=\int_0^l\left(u\frac{d^4v}{dx^4}-v\frac{d^4u}{dx^4}\right)dx$$

$$=u\frac{d^3v}{dx^3}-v\frac{d^3u}{dx^3}+\frac{dv}{dx}\frac{d^2u}{dx^2}-\frac{du}{dx}\frac{d^2v}{dx^2}, \quad . \quad . \quad . \quad (10)$$

as may be found by integrating by parts. The integrated terms are to be taken between the limits, and vanish in each case,

whether the end be clamped, free, or supported. Hence

$$(p'^2-p^2)\int_0^l uvdx=0, \quad . \quad . \quad . \quad . \quad (11)$$

from which we infer that any two normal functions corresponding to different periods are conjugate.

This process may perhaps seem as simple as could be expected; but a little consideration will show that in the derivation of (11) we have in fact retraced the steps by which the ordinary differential equation was itself proved, *and that the true foundation of* (11) *is the variational equation from which we originally started.*

In the equation referred to, namely

$$\delta V+\int\rho\ddot{y}\delta ydx=0,$$

δV is a symmetrical function of y and δy, as may be seen either from its form in the present problem, or by the general theorem proved below. Suppose that y refers to the motion corresponding to a normal function u, so that $\ddot{y}+p^2y=0$, while δy is proportional to another normal function v; then

$$\delta V=p^2\int_0^l \rho uvdx.$$

Again, if we suppose, as we are equally entitled to do, that y varies as v and δy as u,

$$\delta V=p'^2\int_0^l \rho uvdx;$$

and thus, as before,

$$(p'^2-p^2)\int_0^l \rho uvdx=0.$$

If p and p' are different,

$$\delta V=\int_0^l uvdx=0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

The symmetrical character of δV is a simple consequence of the fact that V is a homogeneous quadratic function of the coordinates. If we suppose that

$$V=\tfrac{1}{2}\{11\}\psi_1^2+\ldots+\{12\}\psi_1\psi_2+\ldots,$$

and let ψ_1 become $\psi_1+\Delta\psi_1$ &c., we find

$$\Delta V=\{11\}\psi_1\Delta\psi_1+\ldots+\{12\}(\psi_1\Delta\psi_2+\psi_2\Delta\psi_1)+\ldots$$
$$+\tfrac{1}{2}\{11\}(\Delta\psi_1)^2+\ldots+\{12\}\Delta\psi_1\Delta\psi_2+\ldots,$$

or, passing to the limit,

$$\delta V=\{11\}\psi_1\delta\psi_1+\ldots+\{12\}(\psi_1\delta\psi_2+\psi_2\delta\psi_1)+\ldots,$$

which is a *symmetrical* function of ψ_1 &c. and $\delta\psi_1$ &c.

The reader will now perceive that the proof of the conjugate property will be scarcely altered if, instead of the bar, we substitute a membrane or plate, whose thickness or material need not be uniform. Denoting the transverse displacement of any point by w, we have as the variational equation,

$$\delta V + \iint \rho \ddot{w} \delta w \, dx \, dy = 0;$$

from which precisely, as before, we deduce

$$(p'^2 - p^2) \iint uv \, dx \, dy = 0.$$

It is, I hope, now clear that great advantage results from the direct employment of the variational equation. The reason of the advantage appears to lie in the fact that every thing required to be known is here embraced in one equation, while the ordinary* differential equation needs to be supplemented by the boundary conditions, which are indeed of the same mechanical importance as itself.

The same method may be applied to the general system. Lagrange's equation of motion for a vibrating system may be written

$$\frac{d}{dt}\left(\frac{dT}{d\dot{\psi}_1}\right)\delta\psi_1 + \ldots + \delta V = 0,$$

where T is the kinetic energy. If the actual motion denoted by ψ_1 &c. be a normal component vibration, each coordinate varies as $\cos pt$, and the general equation may be written

$$-p^2 \delta T_1 + \delta V = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where T_1 is the same function of ψ_1, ψ_2, &c. that T is of $\dot{\psi}_1$ &c. By the same reasoning as before we infer that if ψ_1 &c. refer to one normal component and $\delta\psi_1$ &c. to another having a different period,

$$\delta T_1 = \delta V = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

LVII. *On a new Spectroscope.* *By* Professor Ch. V. Zenger†.

In the spectroscopes usually employed the maximum of dispersion is attained by the combination of several prisms; but this entails a very great loss of light by absorption and reflection at the numerous surfaces of the prisms. It is obvious that feeble intensity of light, or, even when the light is strong, feeble physiological action of the red and violet rays of the spectrum much interferes with exactness and distinctness of vision, especially in the spectral analysis of starlight.

* *Ordinary* is here used in opposition to *variational*, not to *partial*.

† Communicated by the Author.

Any contrivance by which this loss could be avoided and yet the same amount of dispersion be secured would be very advantageous for spectrum-observations on feeble, and even on strong light.

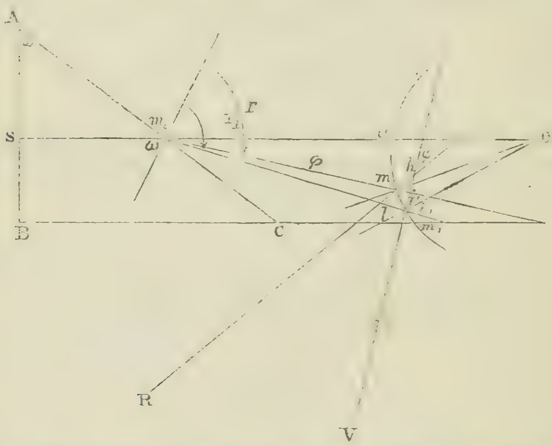
For this purpose I employ only a single prism of the least-absorbent material and the greatest dispersion attainable. It seemed to me that a prism of pure transparent ice-spar would best fulfil this condition, as its index of refraction is :—

$$n_b = 1{\cdot}6531,$$
$$n_h = 1{\cdot}6833,$$
$$\Delta n = 0{\cdot}0302\,;$$

so that its dispersion is greater than that of crown glass, and but little less than that of flint glass, while it absorbs very little of the extreme red and violet rays.

To explain the action of the new spectroscope, let A B C (fig. 1) be the section of a rectangular prism of spar; the solar

Fig. 1.



ray Sm_0 falling at right angles on the surface A B, the angle of incidence ω is equal to the angle of the prism. After refraction the red ray falls in the direction $m_0\,m$ on the surface of a cylindrical mirror and is reflected from it in the direction m R; and the violet ray will be reflected, at a greater angle, in the direction m_1 V.

The diagram shows at once that the effect of the cylindrical surface $a\,m\,m_1$ is to produce an increase of the angle of dispersion $m\,m_0\,m_1 = \phi$ to R h V $= \psi$. It is easy to account for the increase, if the angle of the refracting prism, the distance between the prism and the mirror, and the radius of curvature of the

mirror be given. In the first place, the angle of dispersion produced by the prism itself will be

$$\phi = r_h - r_b,$$

equal to the difference of the angle of refraction for the red and the violet rays:

$$\sin r_h = n_h \,.\, \sin \omega\,;$$
$$\sin r_b = n_b \,.\, \sin \omega.$$

If the angle of the prism is not too large, the sines may be replaced by the angles:

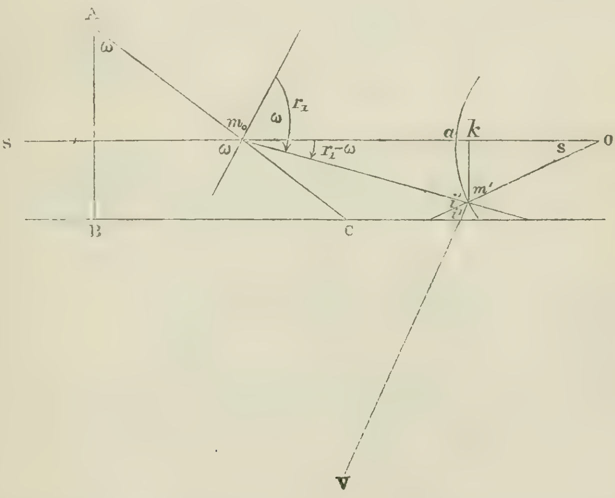
$$r_h = n_h \omega\,;$$
$$r_b = n_b \omega\,;$$
$$r_h - r_b = (n_h - n_b)\,\omega,$$
$$\phi = (n_h - n_b)\,\omega = \omega \,.\, \Delta n. \quad . \quad . \quad . \quad (1)$$

The angle of incidence (i) at a given distance between the mirror and of the prism (fig. 2) is

$$i = r - \omega + \rho,$$

ρ being the angle of aperture of the mirror.

Fig. 2.



In the tringle $m_0 m' o$,

$$m_0 o : m'o = \sin i : \sin (r - \omega)\,;$$

or approximately

$$m_0 o : m'o = i : r - \omega\,;\;\; r - \omega = (n - 1)\,\omega.$$

$m_0o = am_0 + ao = a + p$, if a be the distance of the prism from the mirror, and p its radius of curvature; we then obtain:—

$$\frac{a+p}{p} = \frac{i}{(n-1)\omega}, \text{ or } i = \left(\frac{a}{r} + 1\right)(n-1)\omega. \quad . \quad (2)$$

For the extreme red and violet rays we get:—

$$i_h = \left(\frac{a}{p} + 1\right)(n_h - 1)\omega, \quad i_h - i_b = \left(\frac{a}{p} + 1\right)(n_h - n_b)\omega,$$

$$i_b = \left(\frac{a}{p} + 1\right)(n_b - 1)\omega, \quad i_h - i_b = \left(\frac{a}{p} + 1\right)\Delta n \,.\, \omega. \quad . \quad (3)$$

The triangle $h\,l\,m_1$ contains the angle ψ of the red and violet rays after the reflection from the mirror; and it measures the increase of dispersion produced by the reflection from the cylindrical surface.

If, therefore, ψ is known, the ratio between the angles ϕ and ψ gives the real increase of dispersion produced in that way.

The angle hlm_1, as an exterior angle of the triangle lm_0m, is $hlm_1 = \phi + 2i$; and $2i$ being the exterior angle of the triangle hlm_1, we obtain:—

$$\psi + \phi + 2i = 2i_1,$$

$$\psi + \phi = 2(i_1 - i),$$

$$i_1 - i = \frac{\psi + \phi}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Combining (3) and (4), we get:—

$$\frac{\psi + \phi}{2} = \left(\frac{a}{p} + 1\right) . \,\omega \,.\, \Delta n,$$

$$\phi = \omega \,.\, \Delta n,$$

$$\psi + \phi = 2\left(\frac{a}{p} + 1\right)\omega \,.\, \Delta n,$$

$$\psi = \left(\frac{2a}{p} + 1\right)\omega \,.\, \Delta n; \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\frac{\psi}{\phi} = \left(\frac{2a}{p} + 1\right). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

To receive the spectrum on the surface of the mirror the angle ρ must be sufficiently great; it is given by the equation

$$\rho = i - (r - \omega); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$\rho = i - (n-1)\omega,$$

or

$$\rho=\left(\frac{a}{p}+1\right)(n-1)\omega-(n-1)\omega;$$

$$\rho=\frac{a}{p}(n-1)\omega.$$

If f be the focal length of the mirror, we have

$$\frac{p}{2}=f, \text{ and } \frac{2}{p}=\frac{1}{f},$$

and we finally obtain:—the angle of dispersion

$$\phi=\omega\,.\,\Delta n; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{I.})$$

the angle of reflection

$$i=\left(\frac{a}{2f}+1\right)(n-1)\omega; \quad . \quad . \quad . \quad . \quad (\mathrm{II.})$$

the difference of the angles of reflection for the extreme red and violet rays

$$\Delta i=\left(\frac{a}{2f}+1\right)\omega\,.\,\Delta n, \quad . \quad . \quad . \quad . \quad (\mathrm{III.})$$

the increase of dispersion being

$$\frac{\psi}{\phi}=(af+1). \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{IV.})$$

The angle of aperture required for the reflecting surface of the mirror becomes

$$\mathrm{S}=\frac{a}{2f}(n-1)\omega. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (\mathrm{V.})$$

The equations (I.) to (V.) give all the dimensions for the construction of the spectroscope itself.

Suppose the prism to be of crown glass, flint glass, sulphide of carbon, or ice-spar, we obtain:—

	Crown glass.	CS_2.	Flint glass.	Ice-spar.
n_h . .	1·5447	1·7025	1·6711	1·6833
n_b . .	1·5243	1·6182	1·6277	1·6531
Δn . .	0·0204	0·0843	0·0434	0·0302

If the increase of the angle of dispersion be ϕ, or $\psi=\phi=21$, the focal length of the mirror $f=2$ centims., $p=4$ centims., then the distance is $a=40$ centims.; $\frac{a}{f}=20$; and the other values may be calculated as follows:—

	Crown glass.	CS_2.	Flint glass.	Ice-spar.
ϕ . .	$0·0204\omega$	$0·0843\omega$	$0·0434\omega$	$0·0302\omega$
ψ . .	$0·4284\omega$	$1·7703\omega$	$0·9114\omega$	$0·6342\omega$
Δi . .	$0·2244\omega$	$0·9273\omega$	$0·4774\omega$	$0·3322\omega$
S . .	$5·243\omega$	$7·025\omega$	$6·711\omega$	$6·531\omega$

It is obvious that ice-spar has the advantage of not absorbing large portions of the extreme red and violet rays, not refracting so much as flint glass or bisulphide of carbon, while yet its dispersion is nearly as much as that of flint glass; it requires therefore not so large an aperture of the mirror as flint glass or bisulphide of carbon.

Supposing the angle of the prism to be 10°, we obtain:—

$$\begin{aligned}\phi &= \overset{\circ}{0}\cdot 302,\\ \psi &= 6\cdot 342,\\ \Delta i &= 3\cdot 322,\\ S &= 65\cdot 31.\end{aligned}$$

For 20°:—

$$\begin{aligned}\phi &= \overset{\circ}{0}\cdot 604,\\ \psi &= 12\cdot 684,\\ \Delta i &= 6\cdot 644,\\ S &= 130\cdot 62.\end{aligned}$$

For more than 30° the reflecting mirror cannot give the whole spectrum at once, unless it has an aperture of 195°·93; and then it would reflect the rays on both sides of the prism, instead of only one as shown in the diagram.

In the case of the angle ω being 10°, the refracting angle of the prism should be nearly 211°, or three prisms must be used of 60°–70° to get the same dispersive power in a common spectroscope.

If the angle be 20°, six prisms of 60°–70° nearly should be used to get the same effect, and so on.

If the angle of incidence be not equal to zero, the dispersion increases to, very approximately,

$$\phi=\left(1+\frac{n^2\sin^2 i}{2}\right)\omega\Delta n,$$

and the preceding equations become:—

$$\phi=\left(1+\frac{n^2\sin^2 i}{2}\right).\,\omega\,.\,\Delta n, \quad \ldots \quad \text{(I.)}$$

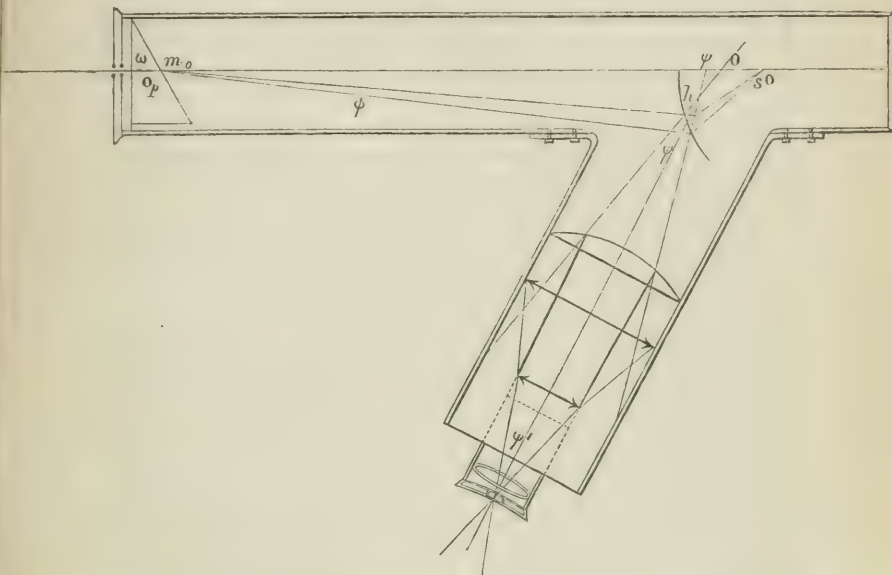
$$\psi=\left(\frac{a}{f}+1\right)\left(1+\frac{n^2\sin^2 i}{2}\right).\,\omega\,.\,\Delta n, \quad . \quad \text{(II.)}$$

$$\Delta i=\left(\frac{a}{2f}+1\right)\left(1+\frac{n^2\sin^2 i}{2}\right)\omega\,.\,\Delta n. \quad . \quad \text{(III.)}$$

There is a practical difficulty in getting exactly cylindrical surfaces; but it is possible to use spherical surfaces instead, if only a narrow zone of the middle part of a convex or concave silvered glass mirror be used. All the formulæ quoted above may be applied to it, and the dispersion augmented without fear of marring the exactness and definition of vision.

The new spectroscope, therefore, consists of a brass tube with a slit at one end and closed at the other end (fig. 3). The slit

Fig. 3.



must be narrow and short, and the prism very near to it, turning by a pinion (p) in its case. The mirror, inclined a certain angle ρ to the axis of the tube, reflects from the other, closed end the spectrum into a brass tube lateral to the former, containing a small telescopic apparatus to receive sharp images of the spectrum reflected by the convex spherical mirror, and to magnify them to ten times the diameter. The mirror is adjusted in such a way as to bring only a narrow zone of its middle part into operation, the rest of the surface being protected by a cover with a narrow slit in the direction of the rays emerging from the prism, and parallel to the reflecting zone of the mirror.

To increase the effect of the mirror, it can be brought nearer to the closed end of the tube, and the side tube moved in the same direction with it. Yet it seems more advisable to fix both at such a distance as will give the greatest separation of the spectral lines attainable without a great loss of light and a diminution of the exactness of the image.

London, October 17, 1873.

LVIII. *Reply to* Professor Challis's *further Remarks* "On the Received Principles of Hydrodynamics."* *By* Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

In his recent paper Professor Challis reiterates, in a slightly different form, his argument derived from the equation $p=\text{funct.}(\rho, v)$, without taking any notice of my refutation of it.

Putting u, v, w for the resolved parts of the velocity, and x, y, z for the coordinates of a particle at the time t, he assumes that the motion in three dimensions of any fluid may be represented by the five following equations; viz.

$$\left.\begin{array}{l} p=f_1(x,y,z,t), \quad \rho=f_2(x,y,z,t), \quad u=f_3(x,y,z,t), \\ \quad v=f_4(x,y,z,t), \quad w=f_5(x,y,z,t)\ddagger. \end{array}\right\} \cdot (\alpha)$$

Eliminating x, y, z, t from equations (α), Professor Challis obtains the equation $\mathrm{F}(p, \rho, u, v, w)=0$, which he assumes to be capable of being "satisfied by an *arbitrary* relation between the quantities —that is, by one which is independent of the particular problem." "The arbitrary condition," says Professor Challis, "has the effect of *defining* the fluid; and evidently the number of different kinds of fluids is unlimited."

I must here point out the very extraordinary character of the admission made by Professor Challis when he asserts that the number of fluids is unlimited, each fluid being defined by the relation between p, ρ, u, v, w prevailing in it—his sole object, so far as I understand it, being to prove that one only of this unlimited number of fluids, and that a fluid in which the highly

* See Phil. Mag. for October last.

† Communicated by the Author.

‡ From this view I must express my dissent, believing that the equations (α) are insufficient to determine the motion in the case in question. Not wishing upon the present occasion, however, to enter upon a fresh field of controversy, I shall content myself with observing that this assumption of Professor Challis contradicts his subsequent assumption that the resultant of the elimination of x, y, z, t from equations (α) will be a perfectly arbitrary function of p, ρ, u, v, w. For in the case of equilibrium the five equations (α) reduce to the pair

$$p=\{f_1(x,y,z,t)\}_{t=0}, \quad \rho=\{f_2(x,y,z,t)\}_{t=0}; \quad \cdot \cdot \cdot \cdot \; (\beta)$$

the elimination between which of z gives

$$p=\text{funct.}(\rho, x, y);$$

which is irreconcilable with Boyle's law in the case of equilibrium, unless we suppose that x and y both disappear along with z in the elimination. But this would require the functions on the right-hand sides of equations (β) to be of a special form; from which it follows that the functions f_1, f_2 must be of a special form, and therefore the result of elimination from them of x, y, t cannot be perfectly arbitrary.

special relation $p=a^2\rho$ obtains has the slightest claim on our attention.

Waiving this consideration, however, I join issue with Professor Challis on the point of our being able to assign to the relation $F(p, \rho, u, v, w)=0$ a definite form which is independent of the particular problem. By assigning a definite form to the arbitrary function $F(p, \rho, u, v, w)=0$, it is not true, as assumed by Professor Challis, that we simply *define* the fluid. We do a great deal more. We define the fluid, and at the same time impose conditions on the motion existing in it.

In proof of this I must again direct attention to the analytical argument of my last paper, which Professor Challis does not appear to have examined.

Confining ourselves to motion in one direction—suppose that we have a cylindrical tube filled with any particular fluid (say air of the mean density of the atmosphere); and suppose that at a given time t we have a disturbance extending over a limited portion of the column. Let the velocity and density throughout the disturbance at the time t follow any law whatsoever consistent with continuity; then I have shown in the paper in question that the pressure prevailing throughout the disturbance at the same time may follow any law whatever consistent with continuity*; from which it is evident that Professor Challis's argument, in proof "that it is allowable to make for fluid in motion the hypothesis that the pressure varies as the density always and at all points," must be fallacious.

I observe that Professor Challis mistakes the points on which I mainly rely for my "objections to the received principles of hydrodynamics."

My main argument is this:—Taking, in the case of motion in one direction,

$$p=f_1(xt), \quad \rho=f_2(xt), \quad v=f_3(xt).$$

I hence derive the equation

$$p=\text{funct.}\ (\rho, v);$$

whence it follows that the equation of motion

$$v=\frac{d^2y}{dt^2}+\frac{1}{D}\frac{dp}{dx}.$$

will be satisfied by the three relations

* It must be borne in mind that the condition of continuity requires that at the limits of the disturbance the velocity shall be zero, the density $=D$, the atmospheric mean density, and the pressure $=a^2D$.

$$p=-\frac{\alpha^2}{\rho}+\phi\left(v+\frac{\alpha}{\rho}\right),$$

$$v+\frac{\alpha}{\rho}=\psi_1\left\{x-\frac{\phi'(\omega)-\alpha}{\mathrm{D}}\cdot t\right\},$$

$$\frac{1}{\rho}+\int\frac{d\omega}{\phi'(\omega)-2\alpha}=\psi_2\left\{x-\frac{\alpha}{\mathrm{D}}\cdot t\right\}.$$

A result more rigorously deduced or more analytically complete is not to be found in the whole range of mathematical physics; and my main objection to the received theory of hydrodynamics is that its upholders must arbitrarily set aside the above relations, being such as have been described, substituting in their room the equation $p=a^2\rho$ with such emasculated results as have been or may be* derived from it—an equation, be it remembered, which is unsupported by a single fact, which is opposed to every sound principle, and as to which all that can be said in its favour is that, in the infancy of the theory, the founders of the theory seized upon it in desperation, amidst the overwhelming difficulties by which they were beset, as the only law of pressure prevailing under any circumstances that up to that time had ever been so much as suggested.

But although I do not rest my main objection to the received principles of hydrodynamics upon the two points to which Professor Challis refers, I regard the views I have put forth in relation to those cases as of importance as showing in a popular manner, conclusively and at a glance as it were, the wholly untenable character of the existing theory of fluid pressure.

As to the first of these cases, Professor Challis writes:—"Mr. Moon founds an argument on the immediate juxtaposition of two densities one of which is double of the other; in other terms, he admits that $\frac{1}{\rho}\frac{dp}{dx}$ may have an infinite value."

I do not know on what ground Professor Challis attributes to me this admission; for, although on entirely different principles†, I am as satisfied as he can be as to the impossibility of

* It is well known that the only solution of the equation of motion under these circumstances which has ever been demonstrated is that derived by Poisson, containing a single arbitrary function, and involving a relation between the velocity and density which it is quite impossible can always or even generally subsist. In a recent Number of the Philosophical Magazine I have shown, as I believe irrefragably, that this solution of Poisson's is the most general which, consistently with the relation $p=a^2\rho$, the equation of motion admits of.

† Professor Challis regards equality of pressure in all directions as a law upon which "the whole of analytical hydrodynamics depends." It is true that the founders of the theory of fluid motion in three dimensions, in their

variation of pressure taking place *per saltum*. Of course if we admit the equation $p=a^2\rho$, variation *per saltum* of density and variation *per saltum* of pressure necessarily go together; but in a discussion as to the truth of that equation it is evident that its validity cannot be assumed.

But though believing variation of pressure *per saltum* to be impossible, I see no more difficulty in supposing a variation of density *per saltum* to occur in a fluid than in supposing two fluids of different densities to be in contact, or that two solids of different densities may be pressed together. Till, therefore, Professor Challis has shown variation of density *per saltum* to be impossible in a fluid, I shall consider myself entitled to insist on all the consequences which, according to the received theory, would flow from its occurrence.

As to the second case, where a weight is suddenly placed upon a piston supported by a vertical column of air, Professor Challis admits that the dilemma I have suggested "is fairly inferred and demands explanation." If it be true that the dilemma is fairly inferred, I think that most persons will regard it as incapable of explanation, and as necessarily involving a modification of the existing theory.

Professor Challis's explanation consists in supposing that "as soon as the weight is added, there will be *a tendency to an excess of pressure on the upper side* [of the stratum immediately underneath the piston] due to the disturbance of pressure in the solid piston."

If the words in italics mean any thing, they mean that there will be an increased pressure of the piston on the air, at the same time that, according to the received theory of pressure in elastic fluids, no increase can have taken place in the pressure of the air upon the piston; which is manifestly a contradiction of the law of the equality of action and reaction. Whatever disturbance of pressure may exist in the other parts of the solid piston, none such can occur at its base consistently with the relation $p=a^2\rho$, until the lapse of an interval of time after the deposition of the additional weight upon the piston.

At the same time I think that a true and complete theory of

early struggles in connexion with the subject, did have recourse to the assumption of the truth of this law; but, so far is it from being an adequate foundation for a general theory of fluid motion, its retention is simple strangulation to the theory, and accounts for the limited and unpromising progeny which trace their origin to it. The true foundation of the theory of fluid motion is the principle of continuity (continuity of motion, that is, as well as continuity of mass), from which it results, as can readily be shown, that any element of the fluid mass will comport itself exactly in the same manner as an element of a rigid body would do under the same circumstances, and may therefore be dealt with accordingly.

the motion which would take place under the circumstances supposed might be evolved by having recourse to considerations similar to those which Professor Challis has suggested—viz. by considering first what would happen if the column of air were a perfectly rigid bar, next by considering the effect of a slight deviation from perfect rigidity, and finally by inferring from that analogy what would take place in the actual case. I hope hereafter to have an opportunity of developing what has been thus briefly sketched.

Avranches, October 11, 1873.

LIX. *On a new Relation between Heat and Static Electricity. By* A. W. Bickerton, *F.C.S., Associate of the Royal School of Mines, Lecturer on Experimental Science, Winchester College, and the Hartley Institution, Southampton**.

ON reading the able paper on the above subject by Dr. Guthrie in the October Number of the Philosophical Magazine, it occurred to me that the relationship might be most satisfactorily and easily explained by the assumption that currents of air passing over an electrified body carry off their electricity. The necessity for the assumption of a coercive force existing between these two physical forces (as suggested in the 'Proceedings of the Royal Society') is thus removed.

The following experiments seem to me to prove in a most decisive manner the correctness of my hypothesis.

Cold air, or cold gas of any kind, even at considerable pressure, when discharged against a charged Peltier's electrometer is incapable of taking away its electricity; but I found that with a stream of hot air the electroscope was rapidly discharged.

In one of the Professor's experiments a heated platinum wire is placed above an electrified body, the body being instantly discharged. I thought it possible that the induced electricity on the heated wire might charge the air in its vicinity with electricity; this charged air passing by attraction down to the electrified body would neutralize it. To ascertain if such a current of air existed, I placed the platinum spiral in the upper part of a gas-jar, a thermometer being passed through the cork so that its bulb was a short distance below the spiral. A brass knob connected with an electric machine was passed up into the jar so as to be close to the thermometer, about 2 inches below the spiral. The knob was also in connexion with a small quadrant electroscope. The machine was worked; and when the

* Communicated by the Author.

spiral was heated, a marked diminution in the indications of the electroscope was observed. After the apparatus had again attained the normal temperature, the current was sent continuously, the rise of the thermometer being noted every minute*, and the brass ball being electified and left unelectrified in alternate minutes. The thermometer indicated on the average a rise of one degree when the ball was not electrified and five degrees when it was electrified—thus proving the existence of a downward current of hot air when the ball was electrified.

Each time the machine was worked the sudden rush of air from the white-hot spiral caused its temperature to be appreciably lowered. On filling the vessel with smoke, the upward convection-currents of heated air showed themselves plainly. On working the machine, the sudden downward rush of smoke from the spiral to the ball was very striking. The smoke disappeared with surprising rapidity when the ball was electrified; it was doubtless burnt in passing over the heated spiral. Repeated experiments proved that the smoke was consumed very much more rapidly when the ball was electrified than when not electrified.

If heated air be the cause of the discharge, the heated wire will possess no power of discharge through rock-salt. This is actually the case; the ignited wire may be placed ever so close to a charged Peltier's electrometer without discharge, if a plate of rock-salt be interposed between them; but remove the salt, and discharge is instantaneous.

An induction-coil giving a spark $\frac{1}{20}$ inch will give a spark $\frac{1}{4}$ inch if the electrodes are at a high temperature. A 1-inch spark becomes $1\frac{1}{4}$ inch under the same conditions.

As it might be said that the lengthening of the spark of the coil was due to rarefaction of the air, the following experiment was made. Platinum wires were fused into the ends of a short piece of combustion-tube. The spark from the small coil passed through a distance of $\frac{3}{16}$ of an inch when the glass, except the fused ends, was strongly heated; the air of course was here almost at the same density. As it is possible to explain the apparent conductivity of glass observed by Dr. Guthrie on the above supposition of air-currents, I sent the spark from the small coil through the heated end of a closed glass tube. I also found that the current from four Grove's cells would pass through $1\frac{1}{2}$ inch of heated glass and deflect a galvanometer.

This power of conductivity is attained at a red heat, and increases enormously with slight increase of temperature. Thus,

* A clock striking minutes with the hand making a complete revolution in the same time was used in all time experiments.

if a small sphere of glass have two platinum electrodes fused into the two sides and it be gradually heated, the galvanometer does not move for some time; but after the first movement is observed, the needle quickly passes up to 90°.

If we assume that hot air possesses the power of carrying off + electricity more readily than —, the whole of Dr. Guthrie's apparently contradictory experiments of the different effects of + and — electricity may be easily explained. Thus a heated body cannot be charged with + electricity, because the heated air carries it off. A body near a heated conductor cannot be charged with —, because it induces + electricity in the heated body; this electrifies the heated air, and the electricity is then carried to the electrified — body and neutralizes it.

Faraday proved that air at ordinary temperatures carried off — electricity most easily.

Hence the experiments of Faraday, Dr. Guthrie, and myself, taken in conjunction, prove the two following principles:—

1st. That at low temperatures — electricity is taken away by air most easily; at certain temperatures both electricities are carried off with equal facility, and at high temperatures + electricity most easily.

2nd. That high-tension electricity can be conveyed away at low temperatures; and as the tension gets lower and lower, it requires the air to be of higher and higher temperatures to carry it off.

To illustrate the second principle, I placed a needle on the knob of a fully charged Peltier's electrometer and left the point within $\frac{1}{100}$ of an inch of an earth-connected brass ball; after two hours the electroscope had only lost half a degree; and on passing a current of hot air, or putting a heated point near it, the electricity was instantly discharged.

The lengthening of the electric arc after the current is once established may be due to these principles, especially from the well-known fact that the part of the + carbon where induction is strongest wears away most rapidly; while the slight wear of the other carbon is as much down the sides as at the point, and hence it remains pointed. It is further confirmed by the fact that if the electric current pass between two electrodes of different oxidizable metals, the space about the light becomes filled with particles of the oxide of the + pole.

LX. *A Discourse on Molecules. By* J. CLERK MAXWELL, *M.A., F.R.S., Professor of Experimental Physics in the University of Cambridge**.

AN atom is a body which cannot be cut in two. A molecule is the smallest possible portion of a particular substance. No one has ever seen or handled a single molecule. Molecular science, therefore, is one of those branches of study which deal with things invisible and imperceptible by our senses, and which cannot be subjected to direct experiment.

The mind of man has perplexed itself with many hard questions. Is space infinite, and if so, in what sense? Is the material world infinite in extent, and are all places within that extent equally full of matter? Do atoms exist, or is matter infinitely divisible?

The discussion of questions of this kind has been going on ever since men began to reason; and to each of us, as soon as we obtain the use of our faculties, the same old questions arise as fresh as ever. They form as essential a part of the science of the nineteenth century of our era as of that of the fifth century before it.

We do not know much about the science organization of Thrace twenty-two centuries ago, or of the machinery then employed for diffusing an interest in physical research. There were men, however, in those days who devoted their lives to the pursuit of knowledge with an ardour worthy of the most distinguished members of the British Association; and the lectures in which Democritus explained the atomic theory to his fellow-citizens of Abdera realized, not in golden opinions only, but in golden talents, a sum hardly equalled even in America.

To another very eminent philosopher, Anaxagoras, best known to the world as the teacher of Socrates, we are indebted for the most important service to the atomic theory which, after its statement by Democritus, remained to be done. Anaxagoras, in fact, stated a theory which so exactly contradicts the atomic theory of Democritus, that the truth or falsehood of the one theory implies the falsehood or truth of the other. The question of the existence or non-existence of atoms cannot be presented to us this evening with greater clearness than in the alternative theories of these two philosophers.

Take any portion of matter, say a drop of water, and observe its properties. Like every other portion of matter we have ever seen, it is divisible. Divide it in two, each portion appears to retain all the properties of the original drop, and among others

* Read before the British Association at Bradford, September 22, 1873.

that of being divisible. The parts are similar to the whole in every respect except in absolute size.

Now go on repeating the process of division till the separate portions of water are so small that we can no longer perceive or handle them. Still we have no doubt that the subdivision might be carried further if our senses were more acute and our instruments more delicate. Thus far all are agreed; but now the question arises, Can this subdivision be repeated for ever?

According to Democritus and the atomic school, we must answer in the negative. After a certain number of subdivisions the drop would be divided into a number of parts each of which is incapable of further subdivision. We should thus in imagination arrive at the atom, which, as its name literally signifies, cannot be cut in two. This is the atomic doctrine of Democritus, Epicurus, and Lucretius, and, I may add, of your lecturer.

According to Anaxagoras, on the other hand, the parts into which the drop is divided are in all respects similar to the whole drop, the mere size of a body counting for nothing as regards the nature of its substance. Hence, if the whole drop is divisible, so are its parts down to the minutest subdivisions, and that without end.

The essence of the doctrine of Anaxagoras is that parts of a body are in all respects similar to the whole. It was therefore called the doctrine of Homoiomereia. Anaxagoras did not, of course, assert this of the parts of organized bodies such as men and animals; but he maintained that those inorganic substances which appear to us homogeneous are really so, and that the universal experience of mankind testifies that every material body without exception is divisible.

The doctrine of atoms and that of homogeneity are thus in direct contradiction.

But we must now go on to molecules. Molecule is a modern word. It does not occur in Johnson's Dictionary. The id it embodies are those belonging to modern chemistry.

A drop of water (to return to our former example) may be divided into a certain number, and no more, of portions similar to each other. Each of these the modern chemist calls a molecule of water. But it is by no means an atom, for it contains two different substances, oxygen and hydrogen; and by a certain process the molecule may be actually divided into two parts, one consisting of oxygen and the other of hydrogen. According to the received doctrine, in each molecule of water there are two molecules of hydrogen and one of oxygen. Whether these are or are not ultimate atoms I shall not attempt to decide.

We now see what a molecule is, as distinguished from an atom.

A molecule of a substance is a small body such that if, on the

one hand, a number of similar molecules were assembled together, they would form a mass of that substance; while, on the other hand, if any portion of this molecule were removed, it would no longer be able, along with an assemblage of other molecules similarly treated, to make up a mass of the original substance.

Every substance, simple or compound, has its own molecule. If this molecule be divided, its parts are molecules of a different substance or substances from that of which the whole is a molecule. An atom, if there is such a thing, must be a molecule of an elementary substance. Since, therefore, every molecule is not an atom, but every atom is a molecule, I shall use the word molecule as the more general term.

I have no intention of taking up your time by expounding the doctrines of modern chemistry with respect to the molecules of different substances. It is not the special but the universal interest of molecular science which encourages me to address you. It is not because we happen to be chemists or physicists or specialists of any kind that we are attracted towards this centre of all material existence, but because we all belong to a race endowed with faculties which urge us on to search deep and ever deeper into the nature of things.

We find that now, as in the days of the earliest physical speculations, all physical researches appear to converge towards the same point, and every inquirer, as he looks forward into the dim region towards which the path of discovery is leading him, sees, each according to his sight, the vision of the same quest.

One may see the atom as a material point, invested and surrounded by potential forces. Another sees no garment of force, but only the bare and utter hardness of mere impenetrability.

But though many a speculator, as he has seen the vision recede before him into the innermost sanctuary of the inconceivably little, has had to confess that the quest was not for him; and though philosophers in every age have been exhorting each other to direct their minds to some more useful and attainable aim, each generation, from the earliest dawn of science to the present time, has contributed a due proportion of its ablest intellects to the quest of the ultimate atom.

Our business this evening is to describe some researches in molecular science, and in particular to place before you any definite information which has been obtained respecting the molecules themselves. The old atomic theory, as described by Lucretius and revived in modern times, asserts that the molecules of all bodies are in motion, even when the body itself appears to be at rest. These motions of molecules are, in the case of solid bodies, confined within so narrow a range that even with our best microscopes we cannot detect that they alter their places at

all. In liquids and gases, however, the molecules are not confined within any definite limits, but work their way through the whole mass, even when that mass is not disturbed by any visible motion.

This process of diffusion, as it is called, which goes on in gases and liquids and even in some solids, can be subjected to experiment, and forms one of the most convincing proofs of the motion of molecules.

Now the recent progress of molecular science began with the study of the mechanical effect of the impact of these moving molecules when they strike against any solid body. Of course these flying molecules must beat against whatever is placed among them; and the constant succession of these strokes is, according to our theory, the sole cause of what is called the pressure of air and other gases.

This appears to have been first suspected by Daniel Bernoulli; but he had not the means which we now have of verifying the theory. The same theory was afterwards brought forward independently by Lesage, of Geneva, who, however, devoted most of his labour to the explanation of gravitation by the impact of atoms. Then Herapath, in his 'Mathematical Physics,' published in 1847, made a much more extensive application of the theory to gases; and Dr. Joule, whose absence from our Meeting we must all regret, calculated the actual velocity of the molecules of hydrogen.

The further development of the theory is generally supposed to have begun with a paper by Krönig, which does not, however, so far as I can see, contain any improvement on what had gone before. It seems, however, to have drawn the attention of Professor Clausius to the subject; and to him we owe a very large part of what has been since accomplished.

We all know that air or any other gas placed in a vessel presses against the sides of the vessel, and against the surface of any body placed within it. On the kinetic theory this pressure is entirely due to the molecules striking against these surfaces, and thereby communicating to them a series of impulses which follow each other in such rapid succession that they produce an effect which cannot be distinguished from that of a continuous pressure.

If the velocity of the molecules is given and the number varied, then since each molecule on an average strikes the sides of the vessel the same number of times, and with an impulse of the same magnitude, each will contribute an equal share to the whole pressure. The pressure in a vessel of given size is therefore proportional to the number of molecules in it—that is, to the quantity of gas in it.

This is the complete dynamical explanation of the fact discovered by Robert Boyle—that the pressure of air is proportional to its density. It shows also that, of different portions of gas forced into a vessel, each produces its own part of the pressure independently of the rest, and this whether these portions be of the same gas or not.

Let us next suppose that the velocity of the molecules is increased. Each molecule will now strike the sides of the vessel a greater number of times in a second; but, besides this, the impulse of each blow will be increased in the same proportion, so that the part of the pressure due to each molecule will vary as the *square* of the velocity. Now the increase of velocity corresponds, on our theory, to a rise of temperature; and in this way we can explain the effect of warming the gas, and also the law discovered by Charles, that the proportional expansion of all gases between given temperatures is the same.

The dynamical theory also tells us what will happen if molecules of different masses are allowed to knock about together. The greater masses will go slower than the smaller ones, so that on an average every molecule, great or small, will have the same energy of motion.

The proof of this dynamical theorem, in whieh I claim the priority, has recently been greatly developed and improved by Dr. Ludwig Boltzmann. The most important consequence which flows from it is that a cubic centimetre of every gas at standard temperature and pressure contains the same number of molecules. This is the dynamical explanation of Gay-Lussac's law of the equivalent volumes of gases. But we must now descend to particulars, and calculate the actual velocity of a molecule of hydrogen.

A cubic centimetre of hydrogen, at the temperature of melting ice and at a pressure of one atmosphere, weighs 0·00008954 gramme. We have to find at what rate this small mass must move (whether altogether or in separate molecules makes no difference) so as to produce the observed pressure on the sides of the cubic centimetre. This is the calculation which was first made by Dr. Joule; and the result is 1859 metres per second. This is what we are accustomed to call a great velocity. It is greater than any velocity obtained in artillery practice. The velocity of other gases is less, as you will see by the Table; but in all cases it is very great as compared with that of bullets.

We have now to conceive the molecules of the air in this hall flying about in all directions at a rate of about seventy miles in a minute.

If all these molecules were flying in the same direction they would constitute a wind blowing at the rate of seventy miles

a minute; and the only wind which approaches this velocity is that which proceeds from the mouth of a cannon. How, then, are you and I able to stand here? Only because the molecules happen to be flying in different directions, so that those which strike against our backs enable us to support the storm which is beating against our faces. Indeed, if this molecular bombardment were to cease even for an instant, our veins would swell, our breath would leave us, and we should literally expire.

But it is not only against us or against the walls of the hall that the molecules are striking. Consider the immense number of them, and the fact that they are flying in every possible direction, and you will see that they cannot avoid striking each other. Every time that two molecules come into collision the paths of both are changed and they go off in new directions. Thus each molecule is continually getting its course altered; so that, in spite of its great velocity, it may be a long time before it reaches any great distance from the point at which it set out.

I have here a bottle containing ammonia. Ammonia is a gas which you can recognize by its smell. Its molecules have a velocity of six hundred metres per second; so that if their course had not been interrupted by striking against the molecules of air in the hall, every one in the most distant gallery would have smelt ammonia before I was able to pronounce the name of the gas. But instead of this, each molecule of ammonia is so jostled about by the molecules of air that it is sometimes going one way and sometimes another, and, like a hare which is always doubling, though it goes a great pace, it makes very little progress. Nevertheless the smell of ammonia is now beginning to be perceptible at some distance from the bottle. The gas does diffuse itself through the air, though the process is a slow one; and if we could close up every opening of this hall so as to make it air-tight, and leave every thing to itself for some weeks, the ammonia would become uniformly mixed through every part of the air in the hall.

This property of gases, that they diffuse through each other, was first remarked by Priestley. Dalton showed that it takes place quite independently of any chemical action between the interdiffusing gases. Graham, whose researches were especially directed towards those phenomena which seem to throw light on molecular motions, made a careful study of diffusion, and obtained the first results from which the rate of diffusion could be calculated.

Still more recently the rates of diffusion of gases into each other have been measured with great precision by Professor Loschmidt, of Vienna.

He placed the two gases in two similar vertical tubes, the

lighter gas being placed above the heavier, so as to avoid the formation of currents. He then opened a sliding valve so as to make the two tubes into one; and after leaving the gases to themselves for an hour or so, he shut the valve, and determined how much of each gas had diffused into the other.

As most gases are invisible, I shall exhibit gaseous diffusion to you by means of two gases (ammonia and hydrochloric acid) which when they meet form a solid product. The ammonia, being the lighter gas, is placed above the hydrochloric acid with a stratum of air between; but you will soon see that the gases can diffuse through this stratum of air and produce a cloud of white smoke when they meet. During the whole of this process no currents or any other visible motion can be detected. Every part of the vessel appears as calm as a jar of undisturbed air.

But, according to our theory, the same kind of motion is going on in calm air as in the interdiffusing gases, the only difference being that we can trace the molecules from one place to another more easily when they are of a different nature from those through which they are diffusing.

If we wish to form a mental representation of what is going on among the molecules in calm air, we cannot do better than observe a swarm of bees, when every individual bee is flying furiously, first in one direction and then in another, while the swarm, as a whole, either remains at rest or sails slowly through the air.

In certain seasons swarms of bees are apt to fly off to a great distance, and the owners, in order to identify their property when they find them on other people's ground, sometimes throw handfuls of flour at the swarm. Now let us suppose that the flour thrown at the flying swarm has whitened those bees only which happened to be in the lower half of the swarm, leaving those in the upper half free from flour. If the bees still go on flying hither and thither in an irregular manner, the floury bees will be found in continually increasing proportions in the upper part of the swarm till they have become equally diffused through every part of it. But the reason of this diffusion is not because the bees were marked with flour, but because they are flying about. The only effect of the marking is to enable us to identify certain bees.

We have no means of marking a select number of molecules of air, so as to trace them after they have become diffused among others, but we may communicate to them some property by which we may obtain evidence of their diffusion.

For instance, if a horizontal stratum of air is moving horizontally, molecules diffusing out of this stratum into those above

and below will carry their horizontal motion with them, and so tend to communicate motion to the neighbouring strata, while molecules diffusing out of the neighbouring strata into the moving one will tend to bring it to rest. The action between the strata is somewhat like that of two rough surfaces, one of which slides over the other, rubbing on it. Friction is the name given to this action between solid bodies; in the case of fluids it is called internal friction, or viscosity.

It is, in fact, only another kind of diffusion—a lateral diffusion of momentum; and its amount can be calculated from data derived from observations of the first kind of diffusion, that of matter. The comparative values of the viscosity of different gases were determined by Graham in his researches on the transpiration of gases through long narrow tubes; and their absolute values have been deduced from experiments on the oscillation of disks by Oscar Meyer and myself.

Another way of tracing the diffusion of molecules through calm air is to heat the upper stratum of the air in a vessel, and to observe the rate at which this heat is communicated to the lower strata. This, in fact, is a third kind of diffusion—that of energy; and the rate at which it must take place was calculated from data derived from experiments on viscosity before any direct experiments on the conduction of heat had been made. Professor Stefan, of Vienna, has recently, by a very delicate method, succeeded in determining the conductivity of air; and he finds it, as he tells us, in striking agreement with the value predicted by the theory.

All these three kinds of diffusion (the diffusion of matter, of momentum, and of energy) are carried on by the motion of the molecules. The greater the velocity of the molecules and the further they travel before their paths are altered by collision with other molecules, the more rapid will be the diffusion. Now we know already the velocity of the molecules; and therefore, by experiments on diffusion, we can determine how far, on an average, a molecule travels without striking another. Professor Clausius, of Bonn, who first gave us precise ideas about the motion of agitation of molecules, calls this distance the mean path of a molecule. I have calculated, from Professor Loschmidt's diffusion-experiments, the mean path of the molecules of four well-known gases. The average distance travelled by a molecule between one collision and another is given in the Table. It is a very small distance, quite imperceptible to us even with our best microscopes. Roughly speaking, it is about the tenth part of the length of a wave of light, which you know is a very small quantity. Of course the time spent on so short a path by such swift molecules must be very small. I have cal-

culated the number of collisions which each must undergo in a second. They are given in the Table, and are reckoned by thousands of millions. No wonder that the travelling power of the swiftest molecule is but small when its course is completely changed thousands of millions of times in a second.

The three kinds of diffusion also take place in liquids; but the relation between the rates at which they take place is not so simple as in the case of gases. The dynamical theory of liquids is not so well understood as that of gases; but the principal difference between a gas and a liquid seems to be that in a gas each molecule spends the greater part of its time in describing its free path, and is for a very small portion of its time engaged in encounters with other molecules; whereas in a liquid the molecule has hardly any free path, and is always in a state of close encounter with other molecules.

Hence in a liquid the diffusion of motion from one molecule to another takes place much more rapidly than the diffusion of the molecules themselves, for the same reason that it is more expeditious in a dense crowd to pass on a letter from hand to hand than to give it to a special messenger to work his way through the crowd. I have here a jar, the lower part of which contains a solution of copper sulphate, while the upper part contains pure water. It has been standing here since Friday, and you see how little progress the blue liquid has made in diffusing itself through the water above. The rate of diffusion of a solution of sugar has been carefully observed by Voit. Comparing his results with those of Loschmidt on gases, we find that about as much diffusion takes place in a second in gases as requires a day in liquids.

The rate of diffusion of momentum is also slower in liquids than in gases, but by no means in the same proportion. The same amount of motion takes about ten times as long to subside in water as in air, as you will see by what takes place when I stir these two jars, one containing water and the other air. There is still less difference between the rates at which a rise of temperature is propagated through a liquid and through a gas.

In solids the molecules are still in motion, but their motions are confined within very narrow limits. Hence the diffusion of matter does not take place in solid bodies, though that of motion and heat takes place very freely. Nevertheless certain liquids can diffuse through colloid solids, such as jelly and gum; and hydrogen can make its way through iron and palladium.

We have no time to do more than mention that most wonderful molecular motion which is called electrolysis. Here is an electric current passing through acidulated water, and causing oxygen to appear at one electrode and hydrogen at the other.

In the space between the water is perfectly calm; and yet two opposite currents of oxygen and of hydrogen must be passing through it. The physical theory of this process has been studied by Clausius, who has given reasons for asserting that in ordinary water the molecules are not only moving, but every now and then striking each other with such violence that the oxygen and hydrogen of the molecules part company and dance about through the crowd, seeking partners which have become dissociated in the same way. In ordinary water these exchanges produce, on the whole, no observable effect; but no sooner does the electromotive force begin to act than it exerts its guiding influence on the unattached molecules, and bends the course of each toward its proper electrode till the moment when, meeting with an unappropriated molecule of the opposite kind, it enters again into a more or less permanent union with it till it is again dissociated by another shock. Electrolysis, therefore, is a kind of diffusion assisted by electromotive force.

Another branch of molecular science is that which relates to the exchange of molecules between a liquid and a gas. It includes the theory of evaporation and condensation, in which the gas in question is the vapour of the liquid, and also the theory of the absorption of a gas by a liquid of a different substance. The researches of Dr. Andrews on the relations between the liquid and the gaseous state have shown us that though the statements in our elementary text-books may be so neatly expressed as to appear almost self-evident, their true interpretation may involve some principle so profound that, till the right man has laid hold of it, no one ever suspects that any thing is left to be discovered.

These, then, are some of the fields from which the data of molecular science are gathered. We may divide the ultimate results into three ranks, according to the completeness of our knowledge of them. To the first rank belong the relative masses of the molecules of different gases, and their velocities in metres per second. These data are obtained from experiments on the pressure and density of gases, and are known to a high degree of precision.

In the second rank we must place the relative size of the molecules of different gases, the length of their mean paths, and the number of collisions in a second. These quantities are deduced from experiments on the three kinds of diffusion. Their received values must be regarded as rough approximations till the methods of experimenting are greatly improved.

There is another set of quantities, which we must place in the third rank, because our knowledge of them is neither precise, as in the first rank, nor approximate, as in the second, but is only

as yet of the nature of a probable conjecture. These are :—the absolute mass of a molecule, its absolute diameter, and the number of molecules in a cubic centimetre. We know the relative masses of different molecules with great accuracy; and we know their relative diameters approximately. From these we can deduce the relative densities of the molecules themselves. So far we are on firm ground.

The great resistance of liquids to compression makes it probable that their molecules must be at about the same distance from each other as that at which two molecules of the same substance in the gaseous form act on each other during an encounter. This conjecture has been put to the test by Lorenz Meyer, who has compared the densities of different liquids with the calculated relative densities of the molecules of their vapours, and has found a remarkable correspondence between them.

Now Loschmidt has deduced from the dynamical theory the following remarkable proportion :—As the volume of a gas is to the combined volume of all the molecules contained in it, so is the mean path of a molecule to one eighth of the diameter of a molecule.

Assuming that the volume of the substance, when reduced to the liquid form, is not much greater than the combined volume of the molecules, we obtain from this proportion the diameter of a molecule. In this way Loschmidt, in 1865, made the first estimate of the diameter of a molecule. Independently of him and of each other, Mr. Stoney in 1868, and Sir W. Thomson in 1870, published results of a similar kind, those of Thomson being deduced not only in this way, but from considerations derived from the thickness of soap-bubbles and from the electric properties of metals.

According to the Table, which I have calculated from Loschmidt's data, the size of the molecules of hydrogen is such that about two millions of them in a row would occupy a millimetre, and a million million million millions of them would weigh between four and five grammes.

In a cubic centimetre of any gas at standard pressure and temperature there are about nineteen million million million molecules. All these numbers of the third rank are, I need not tell you, to be regarded as at present conjectural. In order to warrant us in putting any confidence in numbers obtained in this way, we should have to compare together a greater number of independent data than we have as yet obtained, and to show that they lead to consistent results.

Thus far we have been considering molecular science as an inquiry into natural phenomena. But though the professed aim of all scientific work is to unravel the secrets of nature, it has another effect, not less valuable, on the mind of the worker.

It leaves him in possession of methods which nothing but scientific work could have led him to invent; and it places him in a position from which many regions of nature, besides that which he has been studying, appear under a new aspect.

The study of molecules has developed a method of its own, and it has also opened up new views of nature.

When Lucretius wishes us to form a mental representation of the motion of atoms, he tells us to look at a sunbeam shining through a darkened room (the same instrument of research by which Dr. Tyndall makes visible to us the dust we breathe), and to observe the motes which chase each other in all directions through it. This motion of the visible motes, he tells us, is but a result of the far more complicated motion of the invisible atoms which knock the motes about. In his dream of nature, as Tennyson tells us, he

> "Saw the flaring atom-streams
> And torrents of her myriad universe,
> Ruining along the illimitable inane,
> Fly on to clash together again, and make
> Another and another frame of things
> For ever."

And it is no wonder that he should have attempted to burst the bonds of Fate by making his atoms deviate from their courses at quite uncertain times and places, thus attributing to them a kind of irrational free will, which on his materialistic theory is the only explanation of that power of voluntary action of which we ourselves are conscious.

As long as we have to deal with only two molecules, and have all the data given us, we can calculate the result of their encounter; but when we have to deal with millions of molecules, each of which has millions of encounters in a second, the complexity of the problem seems to shut out all hope of a legitimate solution.

The modern atomists have therefore adopted a method which is, I believe, new in the department of mathematical physics, though it has long been in use in the section of statistics. When the working members of Section F get hold of a report of the census or any other document containing the numerical data of economic and social science, they begin by distributing the whole population into groups, according to age, income-tax, education, religious belief, or criminal convictions. The number of individuals is far too great to allow of their tracing the history of each separately; so that, in order to reduce their labour within human limits, they concentrate their attention on a small number of artificial groups. The varying number of individuals in each group, and not the varying state of each individual, is the primary datum from which they work.

This of course is not the only method of studying human nature. We may observe the conduct of individual men and compare it with that conduct which their previous character and their present circumstances, according to the best existing theory, would lead us to expect. Those who practise this method endeavour to improve their knowledge of the elements of human nature in much the same way as an astronomer corrects the elements of a planet by comparing its actual position with that deduced from the received elements. The study of human nature by parents and schoolmasters, by historians and statesmen is therefore to be distinguished from that carried on by registrars and tabulators, and by those statesmen who put their faith in figures. The one may be called the historical, and the other the statistical method.

The equations of dynamics completely express the laws of the historical method as applied to matter; but the application of these equations implies a perfect knowledge of all the data. But the smallest portion of matter which we can subject to experiment consists of millions of molecules, not one of which ever becomes individually sensible to us. We cannot, therefore, ascertain the actual motion of any one of these molecules; so that we are obliged to abandon the strict historical method and to adopt the statistical method of dealing with large groups of molecules.

The data of the statistical method as applied to molecular science are the sums of large numbers of molecular quantities. In studying the relations between quantities of this kind we meet with a new kind of regularity, the regularity of averages, which we can depend upon quite sufficiently for all practical purposes, but which can make no claim to that character of absolute precision which belongs to the laws of abstract dynamics.

Thus molecular science teaches us that our experiments can never give us any thing more than statistical information, and that no law deduced from them can pretend to absolute precision. But when we pass from the contemplation of our experiments to that of the molecules themselves, we leave the world of chance and change, and enter a region where every thing is certain and immutable.

The molecules are conformed to a constant type with a precision which is not to be found in the sensible properties of the bodies which they constitute. In the first place, the mass of each individual molecule and all its other properties are absolutely unalterable. In the second place, the properties of all molecules of the same kind are absolutely identical.

Let us consider the properties of two kinds of molecules, those of oxygen and those of hydrogen.

We can procure specimens of oxygen from very different sources—from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different; and if during thousands of years difference of circumstances could produce difference of properties, these specimens of oxygen would show it.

In like manner we may procure hydrogen from water, from coal, or, as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water.

Now, if during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved.

But we have another and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements; and when these are excited, it emits rays, the wave-length of which is a measure of the time of vibration of the molecule.

By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained not only that molecules taken from every specimen of hydrogen in our laboratories have the same set of periods of vibration, but that light having the same set of periods of vibration is emitted from the sun and from the fixed stars.

We are thus assured that molecules of the same nature as those of our hydrogen exist in those distant regions, or at least did exist when the light by which we see them was emitted.

From a comparison of the dimensions of the buildings of the Egyptians with those of the Greeks, it appears that they have a common measure. Hence, even if no ancient author had recorded the fact that the two nations employed the same cubit as a standard of length, we might prove it from the buildings themselves. We should also be justified in asserting that at some time or other a material standard of length must have been carried from one country to the other, or that both countries had obtained their standards from a common source.

But in the heavens we discover by their light, and by their light alone, stars so distant from each other that no material thing can ever have passed from one to another; and yet this light, which is to us the sole evidence of the existence of these

distant worlds, tells us also that each of them is built up of molecules of the same kinds as those which we find on earth. A molecule of hydrogen, for example, whether in Sirius or in Arcturus, executes its vibrations in presisely the same time.

Each molecule, therefore, throughout the universe bears impressed on it the stamp of a metric system as distinctly as does the metre of the Archives at Paris or the double royal cubit of the Temple of Karnac.

No theory of evolution can be formed to account for the similarity of molecules; for evolution necessarily implies continuous change, and the molecule is incapable of growth or decay, of generation or destruction.

None of the processes of nature, since the time when nature began, have produced the slightest difference in the properties of any molecule. We are therefore unable to ascribe either the existence of the molecules or the identity of their properties to the operation of any of the causes which we call natural.

On the other hand, the exact equality of each molecule to all others of the same kind gives it, as Sir John Herschel has well said, the essential character of a manufactured article, and precludes the idea of its being eternal and self-existent.

Thus we have been led, along a strictly scientific path, very near to the point at which science must stop. Not that science is debarred from studying the internal mechanism of a molecule which she cannot take to pieces, any more than from investigating an organism which she cannot put together. But in tracing back the history of matter, science is arrested when she assures herself, on the one hand, that the molecule has been made, and on the other, that it has not been made by any of the processes we call natural.

Science is incompetent to reason upon the creation of matter itself out of nothing. We have reached the utmost limit of our thinking faculties when we have admitted that because matter cannot be eternal and self-existent it must have been created.

It is only when we contemplate, not matter in itself, but the form in which it actually exists, that our mind finds something on which it can lay hold.

That matter, as such, should have certain fundamental properties—that it should exist in space and be capable of motion, that its motion should be persistent, and so on, are truths which may, for any thing we know, be of the kind which metaphysicians call necessary. We may use our knowledge of such truths for purposes of deduction; but we have no data for speculating as to their origin.

But that there should be exactly so much matter and no more

in every molecule of hydrogen is a fact of a very different order. We have here a particular distribution of matter—a *collocation* —to use the expression of Dr. Chambers, of things which we have no difficulty in imagining to have been arranged otherwise.

The form and dimensions of the orbits of the planets, for instance, are not determined by any law of nature, but depend upon a particular collocation of matter. The same is the case with respect to the size of the earth, from which the standard of what is called the metrical system has been derived. But these astronomical and terrestrial magnitudes are far inferior in scientific importance to that most fundamental of all standards which forms the base of the molecular system. Natural causes, as we know, are at work which tend to modify, if they do not at length destroy, all the arrangements and dimensions of the earth and the whole solar system. But though in the course of ages catastrophes have occurred and may yet occur in the heavens, though ancient systems may be dissolved and new systems evolved out of their ruins, the molecules out of which these systems are built—the foundation stones of the material universe—remain unbroken and unworn. They continue this day as they were created—perfect in number and measure and weight; and from the ineffaceable characters impressed on them we may learn that those aspirations after accuracy in measurement, truth in statement, and justice in action, which we reckon among our noblest attributes as men, are ours because they are essential constituents of the image of Him who in the beginning created, not only the heaven and the earth, but the materials of which heaven and earth consist.

TABLE of Molecular Data.

		Hydrogen.	Oxygen.	Carbonic oxide.	Carbonic acid.
Rank I.	Mass of molecule (hydrogen = 1)	1	16	14	22
	Velocity, mean (square metres per second), at 0° C.	1859	465	497	396
Rank II.	Mean path, tenth-metres......	965	560	482	379
	Collisions in a second (millions)	17750	7646	9489	9720
Rank III.	Diameter, tenth-metres.........	5·8	7·6	8·3	9·3
	Mass, twenty-fifth grammes...	46	736	644	1012

TABLE of Diffusion.

$\frac{(\text{Centimetre})^2}{\text{Second}}$ measure.

	Calculated.	Observed.	
H & O	0·7086	0·7214	Diffusion of matter observed by Loschmidt.
H & CO	0·6519	0·6422	
H & CO^2	0·5575	0·5558	
O & CO	0·1807	0·1802	
O & CO^2	0·1427	0·1409	
CO & CO^2	0·1386	0·1406	
H	1·2990	1·49	Diffusion of momentum (Graham and Meyer).
O	0·1884	0·213	
CO	0·1748	0·212	
CO^2	0·1087	0·117	
Air	....	0·256	Diffusion of temperature observed by Stefan.
Copper	....	1·077	
Iron	....	0·183	
Cane-sugar in water	....	0·00000365	Voit.
(or in a day	....	0·3144)	
Salt in water	....	0·00000116	Fick.

LXI. *On the Differential Galvanometer.* *By* OLIVER HEAVISIDE, *Newcastle-on-Tyne**.

THE great similarity between the systems of resistance-measuring by means of the differential galvanometer and Wheatstone's bridge, the latter having been probably suggested by the former, must have struck every one who has had any thing to do with them. In each case do we make one resistance a fourth proportional to three others, and, knowing the three, deduce the fourth. As in the bridge for every resistance to be measured there is a certain arrangement of the three other sides which gives the most sensitive balance, so with the differential galvanometer there must be a best arrangement for any particular case, which it is the object of this paper to point out.

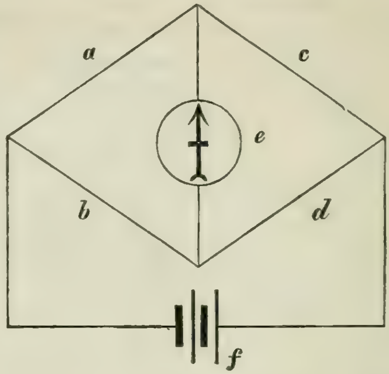
The expression for the strength of the current through the galvanometer in Wheatstone's bridge is

$$\mathrm{E}=\frac{v\cdot\dfrac{ad-bc}{a+b+c+d}}{\left\{\dfrac{(a+b)(c+d)}{a+b+c+d}+e\right\}\cdot\left\{\dfrac{(a+c)(b+d)}{a+b+c+d}+f\right\}}\quad . \qquad (1)$$

(where v is the electromotive force of the battery, E the current

* Communicated by the Author.

through e, and the resistances as in the diagram) when at a balance and therefore $ad-bc$ a vanishing quantity.



To deduce from this the expression for the force acting on the needle in the differential galvanometer, let a and b be the two coils. Then, in the first place, by Kirchhoff's rule,

$$\mathrm{A}a-\mathrm{B}b=\mathrm{E}e,$$

where A, B, E are the currents in a, b, and e respectively; and next, that as e is absent in the differential-galvanometer arrangement, we must make e infinite. Therefore, multiplying (1) by e and making $e=\infty$, we obtain

$$\mathrm{A}a-\mathrm{B}b=\frac{v\cdot\dfrac{ad-bc}{a+b+c+d}}{\dfrac{(a+c)(b+d)}{a+b+c+d}+f} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

for the differential galvanometer. This can, of course, be obtained independently of any consideration of Wheatstone's bridge, but makes it evident that the best arrangement of the differential galvanometer with a given battery may be derived from the Wheatstone's-bridge formulæ by making e infinite in them.

These formulæ are as follows (Phil. Mag. February 1873). When c, d, e, and f are fixed,

$$\left.\begin{aligned} a&=\sqrt{\frac{c}{d}\cdot\frac{cd+df+fc}{c+d+e}\cdot e}, \\ b&=\sqrt{\frac{d}{c}\cdot\frac{cd+df+fc}{c+d+e}\cdot e}; \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (3)$$

and if c be not arbitrarily fixed,

$$\left.\begin{aligned} a&=\sqrt{ef}, \\ b&=\sqrt{de\cdot\frac{d+f}{d+e}}, \\ c&=\sqrt{df\cdot\frac{d+e}{d+f}}, \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

which is the most sensitive arrangement possible with a given

galvanometer and battery. Finally, if the best resistances for the galvanometer and battery are also to be employed, we must make every branch of the bridge of the same resistance, viz. that of d, the resistance to be measured.

Making $e=\infty$ in (3) and (4), they become

$$\left.\begin{aligned} a &= \sqrt{\frac{c}{d}(cd+df+fc)}, \\ b &= \sqrt{\frac{d}{c}(cd+df+fc)}, \end{aligned}\right\} \quad \ldots \ldots \quad (5)$$

and

$$\left.\begin{aligned} a &= \infty, \\ b &= \sqrt{d(d+f)}, \\ c &= \infty, \end{aligned}\right\} \quad \ldots \ldots \ldots \quad (6)$$

which may also be obtained by differentiation from (2).

These formulæ, (5) and (6), would not be of any particular use if we had no means of varying at will the resistance of the coils a and b. This cannot be accomplished directly without considerable complication; but by means of shunts the same end may be reached. Thus, using (5), if the coils of our galvanometer have resistances greater than those best suited for the particular resistance to be measured, by means of shunts we may reduce their resistances to the required extent. And here a remarkable peculiarity presents itself. In general when a galvanometer is shunted, its resistance and sensibility are reduced and in the same proportion. Not so with the differential galvanometer; for its sensitiveness will be increased or reduced by shunts according as the normal resistances of its coils are greater or less than their best values in the particular case under consideration. The accuracy of (5) may be easily verified experimentally.

Proceeding to examine (6), we meet practical impossibilities, and can only carry it out by making the resistance of the coil a as great as possible by not shunting it. But as the values of b and c in (6) correspond to $a=\infty$, it will be necessary to find their values when $a=a$. Therefore, regarding both a and d as constant, and b and c variable, subject to the condition $ad-bc=0$, we shall find by differentiating (3) that

$$\left.\begin{aligned} c &= \sqrt{ad \cdot \frac{a+f}{d+f}}, \\ b &= \sqrt{ad \cdot \frac{d+f}{a+f}} \end{aligned}\right\} \quad \ldots \ldots \quad (7)$$

gives the most sensitive arrangement for measuring a resistance d with a battery whose resistance is f.

It is an evident conclusion that differential galvanometers intended for measuring resistances comprised within wide limits, both high and low, should have coils of long fine wire, having necessarily a high resistance; for a galvanometer with coils of short and thick wire is only suitable for measuring small resistances; whereas if it have coils of fine wire it is suitable for both high and low resistances—for the latter by shunting.

LXII. *On certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat.* *By* W. F. Barrett, *F.C.S., Professor of Experimental Physics in the Royal College of Science, Dublin*.*

IN the 'Proceedings of the Royal Society' for January 28, 1869, Mr. Gore published the important fact, that when an iron wire is heated to bright incandescence and then allowed to cool, *a momentary elongation*, or, as Mr. Gore believed, diminution of cohesion, of the wire occurs just after it has begun to contract by cooling. The main points in Mr. Gore's paper are as follows:—A thin iron wire fixed at one end to a binding-screw is attached at the other to an index which multiplies any motion of the wire; the wire is strained horizontally by a feeble spring; and matters are so arranged that the wire can be heated by an electric current or by a row of gas-jets. When heated, the wire expands and the spring pulls the index over. A sketch of the instrument is given in the Philosophical Magazine for July 1869. Mr. Gore states that *no* anomalous action is observed on *heating* the wire to bright incandescence; but when the heating is discontinued and cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk or kick, indicating a momentary elongation of the wire during the progress of its contraction. This effect is perfectly certain, and always occurs at this particular temperature. Mr. Gore states that iron wire of a certain thinness and a certain tension of the spring is necessary, and that the phenomenon is apparently *confined to cooling iron,* no such change being evident during the heating or cooling of wires drawn from the wide range of other metals he has examined. Further, Mr. Gore has investigated the production of induced currents during the cooling of magnetized iron bars, one portion of which had been heated to redness; and the result showed that the iron bar "suddenly increased in magnetic capacity during cooling at a particular temperature of moderate red heat."

Having occasion to show Mr. Gore's discovery in the course of a lecture delivered some eighteen months ago to the Dublin

* Communicated by the Author.

Royal Society, Mr. Gore kindly furnished me with his own apparatus. By attaching to the movable cross piece a light mirror, from which a brilliant ray of light was reflected to a scale on a distant wall, the effect sought was not only vastly magnified, but one or two new facts also revealed themselves. (i) During the heating of the wire a slight and momentary retrogression of the beam was noticed at the temperature corresponding to the powerful jerk that occurred on cooling: some smaller tremblings of the beam were noticed at higher and lower temperatures; but these seemed due to irregular heating and cooling. (ii) It was evident that the anomalous deportment of the iron occurred approximately at the critical temperature when iron undergoes its principal magnetic change.

Mr. Gore having stated in a letter to me, written in May 1872, that he had no intention at present of making any more experiments in the direction of his discovery, and adding the subject was quite open to me, I felt at liberty to pursue the inquiry thus suggested. It was not, however, till this autumn that I could find the necessary leisure; and the following results were then obtained.

My best thanks are here due to Professor Guthrie for his hearty welcome to use his laboratory at South Kensington, where the experiments have been conducted.

I.

Employing twenty Grove cells, I have had no difficulty in obtaining this anomalous behaviour with *moderately thick* iron wires. These have the advantage of allowing the effect to be studied more leisurely, the phenomenon sought for occurring several seconds after the interruption of the current. The temperature at which the momentary jerk occurs seems to be lower in thick wires than in thin ones, the critical point being a moderately bright or cherry-red heat in thin iron wire, say No. 23, and a very dull red heat in thick wire, say No. 20; the latter wire is, in fact, in the stage just preceding obscurity when the effect occurs. The internal temperature of the thicker wires is no doubt masked by the cooling of their surface, whereas in thin wires the cooling throughout is extremely rapid, and moreover the transitions of temperature cannot be so well noted.

II.

With No. 21 hard iron wire I have had no difficulty in obtaining the jerk during heating. In this case the movement is in the reverse direction of that which occurs during cooling; that is to say, it indicates a momentary *retraction*, occurring, as closely as can be judged, at the same temperature at which the *elongation* takes place in cooling.

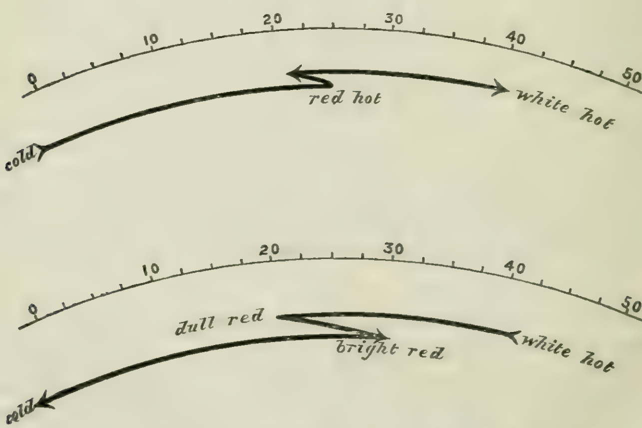
With a steel wire 25 centims. long and No. 22 B wire gauge the following observations were made—a battery of ten Grove cells being used, capable of raising this length of wire to a bright white heat. The index stood at 0 on the scale before contact was made, the wire being cold. After contact, as the wire became heated, the index regularly passed to 24; here it promptly retreated to 22, then steadily passed onwards to 34, the wire now glowing white-hot. Breaking contact, the index returned regularly to 20, then rose suddenly to 27, after which it continued its backward course till it finally rested at 2, the wire now being cold again. The action of the spring stretches the wire when hot, hence the index does not return to zero. Allowing for this stretching, the figures would be proportionally lower where the jerk occurs on cooling, viz. 18 to 25.

Here are two more out of many experiments with the same wire:—

1. Wire cold; contact made; index rose from 0 to 25, jerked back to 23, then rose to 33; wire bright red.
   Wire bright red; contact broken; index fell from 33 to 19, jerked forward to 25, then fell to 4; wire cold.
2. Wire cold; contact made; index rose from 0 to 25, jerked back to $23\frac{1}{2}$, then rose to 32; wire bright red.
   Wire bright red; contact broken; index fell from 32 to 20, jerked forward to $24\frac{1}{2}$, then fell to 4; wire cold.

The following diagram (fig. 1) illustrates the motion of the index on heating and cooling the wire.

Fig. 1.



Releasing the tension of the spring, the forward motion on

cooling is, as might be expected, much lessened, whilst the jerk back is scarcely affected. Increasing the tension of the spring, the forward jerk is correspondingly increased and the backward jerk diminishes, and can be made to disappear.

III.

Is this anomalous action, then, due to a momentary change in the cohesion of the wire? If so, at a certain point during the progress of *heating*, the molecules of iron have a sudden *accession* in elasticity, and at an approximately corresponding point during *cooling*, they incur a sudden *loss* in elasticity. If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling*. Fixing one end of the wire and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen; the sudden outward thrust on cooling is very conspicuous. Heating the wire by gas-flames, the same result is given.

All kinds of iron do not exhibit this behaviour; and some show it in a more or less marked degree. I have not been able to detect any change, in heating or cooling, in certain specimens of good soft iron wire; but in hard iron wire, and notably in steel wire, it is very apparent. The wire, moreover, requires to be raised to a very high temperature before the jerk is seen on cooling. I have not observed the momentary elongation on cooling when the wire has only been heated to a point *just beyond* that at which it would otherwise occur. The behaviour of iron wires of different degrees of purity and of widely different thicknesses are points I hope to examine in a subsequent inquiry. I may here also mention that the precise magnetic condition of the iron at the moment at which the jerk occurs, together with its electric resistance and its thermo-electric position †, are questions upon which I have already made some experiments, but not enough to justify the publication of any results at present.

* A striking lecture experiment may be made by simply stretching some harpsichord-wire between two supports, and heating the wire to whiteness by a current. On allowing the wire to cool, it gradually straightens itself till just as it reaches the point of obscurity, when it suddenly drops for an instant. It is extraordinary that this action has not been frequently observed.

† Professor Tait's remarkable investigation on this point is alluded to subsequently.

IV.

On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.* Necessarily no change of this kind can be observed on heating; but the reglowing of the wire on cooling is most uniform and conspicuous*. The wire must first be heated to whiteness; and then, being allowed to cool, just as it reaches a point of barely visible redness a sudden cherry-red glow takes place, passing as a wave of heat from one end of the wire to the other, or from both ends to the centre. The measured progress of this wave of temperature along the wire is extremely beautiful to observe. On first sending the current through the wire, the heating begins at one extremity and runs along to the other; on breaking contact, this reheating sweeps along the wire in the contrary direction. This peculiar movement, therefore, may be caused by the unequal thickness of the wire; though I do not think this is the explanation, as the reheating would then move in the same direction as the heating (namely, from the thinner to the thicker parts of the wire), and this is not the case. I hope shortly to investigate this further.

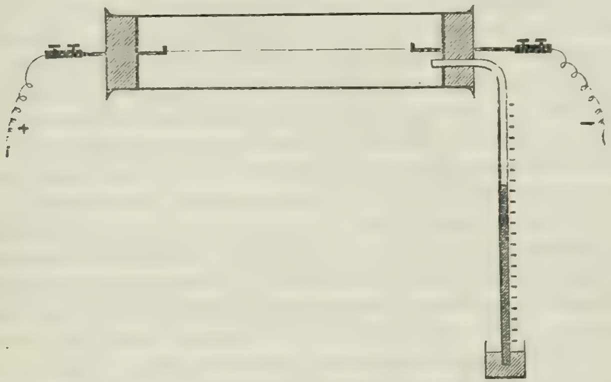
When the wire is heated by a row of gas-flames, the same results take place, although the heating by the battery is a far neater and more satisfactory way.

It is a real accession of temperature, a sudden increase in thermal as well as luminous radiation. This is evident from the following experiment. A wide glass tube (fig. 2) was fitted with corks at each end, so that the iron wire could be enclosed air-tight within the tube. At one end the cork was perforated to allow the insertion of a narrow glass tube bent at right angles, the lower end of which dipped into coloured water. On heating the wire to whiteness by the current, some of the enclosed air was expelled, and on breaking contact the liquid rushed up the tube, but midway suddenly stopped in its course, and was depressed some two inches. At this moment the assistant, who was watching the wire, gave notice the wire drooped and glowed again. There is no difficulty in repeating this experi-

* Nevertheless during heating I thought I detected a momentary pause in the progress of the reddening of the wire, just after incandescence had been reached.

ment, nor in exhibiting its action to a large class. Wherever the momentary expansion of the wire is feeble or absent, there

Fig. 2.



likewise this *recalescence*, as it might perhaps be termed, is also feeble or absent. I am anxious to procure wires of nickel and cobalt; for, from the reasons set forth in another paper, the action might certainly be expected to occur in those metals as well as in iron, probably with nickel at a lower and with cobalt at a higher temperature than in the case of iron.

V.

Besides the molecular changes here detailed, there are indications of the existence of other disturbances during the heating and cooling of an iron wire, notably *the emission of a peculiar dry crackling sound,* like the crepitation that occurs on magnetizing and demagnetizing iron; and this, too, occurs at the critical temperature. But so many matters of interest have arisen during this investigation, that the present paper can only be regarded as the results of a preliminary inquiry.

VI.

The molecular disturbances to which iron is thus seen to be subject, at a particular temperature, are no doubt associated with an even wider range of phenomena than are here indicated. Professor Tait's experiments, which were read by me after most of the foregoing facts had been obtained, show that iron exhibits a most remarkable and anomalous *thermo-electric* deportment at a red heat. In his Rede Lecture* that eminent physicist points out that "the cause of this is that while, as Sir W. Thomson discovered, the specific heat of electricity in iron is negative at ordinary temperatures, it becomes *positive* at some temperature

* Nature, June 12, 1873.

near low red heat, and remains positive till near the melting-point of iron, where it appears possible from some of my experiments that it may again change sign." And further on Professor Tait suggests the idea "that iron becomes, as it were, a different metal on being raised above a certain temperature. This may possibly have some connexion with the ferricum and ferrosum of the chemist, with the change of magnetic properties of iron, and of its electric resistance at high temperatures." Professor Tait adds the interesting fact that he has found an anomalous thermo-electric behaviour in *nickel* similar to that in iron, and, as one might venture to anticipate, at a much lower temperature.

Thus two separate lines of inquiry have converged on the same point—namely, that a profound molecular disturbance takes place in iron at a low red heat. In connexion with future theories of magnetism, this fact is likely to be of considerable importance, inasmuch as it seems probable that this disturbance is confined to the magnetic metals, and that it occurs at or about the temperature when they leave or reenter this condition.

LXIII. *On the Relationship of the Magnetic Metals.* *By* W. F. Barrett, *F.C.S.**

THE remarkable similarity in the chemical and physical properties of the magnetic metals has no doubt often attracted attention; but I am not aware that any definite collation of these properties has ever been made. This I propose briefly to do in the following paper. The extraordinary homology these metals are thus seen to exhibit furnishes instructive evidence concerning the molecular state of a magnet.

By magnetic metals I mean those metals which possess magnetic properties under ordinary circumstances—namely, iron, nickel, and cobalt.

First we will compare their physical characteristics. The *specific gravity* of the thirty-eight known metals ranges from lithium 0·59, to platinum 21·5, a difference of nearly 21; whereas the specific gravity of iron is 7·8, nickel 8·3, and cobalt 8·5, an extreme difference of only 0·7. The *specific heat* of these three metals is also nearly identical; and their *atomic heat* is the same. Their *conductivity for sound* is almost absolutely the same; and so far as their heat and electric conductivity have been determined they are also alike. Their *dilatation by heat* is the same, and so also is the amount they lengthen by mechanical strain. They belong, I believe, to the same system of *crystallization,* namely the monometric, though too little is known

* Communicated by the Author, having been read before the British Association at Bradford, September 1873.

on this point. The enormous *cohesive power* of iron, nickel, and cobalt in the solid state signalizes these substances as the most *tenacious* of metals. To overcome this cohesion a very high and somewhat similar temperature is required, and their *melting-point* is only exceeded by the platinum group of metals. Their refractory character renders them not volatile even at the temperature of the hottest furnace. When, however, they are volatilized by means of the electric spark, their incandescent vapours yield a *spectrum* which has a close and curious resemblance. This teaches us that the molecules of these bodies, freed from the thrall of cohesion, vibrate in periods which are closely akin.

A comparison of the *chemical* properties of the same metals furnishes a similar result. The ratio of the combining weight of the metallic elements ranges from lithium 7, to bismuth as 210, or a difference of 203. When we compare the magnetic metals, we find the combining weight of iron is 56·0, nickel 58·5, and cobalt 58·5, or a difference of only 2·5. Chemists class these three metals in the same group from the similarity of their chemical behaviour, and also the identity of their combining energy or atomicity.

In strong nitric acid iron becomes endowed with a so-called passive condition, not acted upon, as it is in the dilute acid. Likewise I find nickel is capable of assuming a passive state in strong nitric acid. Cobalt, it is true, was violently acted upon under similar circumstances; but that, I believe, was due to the fact that the cobalt contained iron largely, and so an electrolytic action was probably set up. I have been unable to obtain pure cobalt—a very difficult matter, I believe.

A series of very similar chemical compounds are formed by these metals, mostly characterized by the brilliancy of their colour. The protosalts of iron are generally bluish green, of nickel emerald green, and of cobalt of a rose-colour. It is moreover a well-known fact that this rose-colour of certain cobalt salts passes into a *bright green* when they are warmed. Now, when the metal cobalt is moderately heated it *increases* in magnetic power, thus differing from its congeners, iron and nickel, which are in their maximum magnetic condition at the ordinary temperature, and at ordinary temperatures present the green-coloured salts*.

What has been said concerning the likeness of iron, nickel, and cobalt, in many respects holds true of *manganese* and *chromium,* also feebly magnetic metals. Placed in the same group

* The therapeutic effect of the salts of these metals one would expect to be somewhat similar; and in confirmation of this I hear that nickel has lately often been used advantageously to replace the medicinal properties of iron.

with the former metals chemically, they are physically characterized by their extraordinary tenacity and difficult fusibility. Manganese has lately been used to replace nickel in the alloy of German silver, and with excellent results I am informed. It is also worthy of note that the compounds of these five metals are conspicuous by the brilliancy of their colours, all their salts exerting a selective absorption on light, and their oxides dissolved in borax yielding well-known and characteristic tints—a comparatively rare feature outside this group.

Further, it is well known that the ores of cobalt and nickel are almost invariably found associated in the earth and with difficulty separated. It is also noteworthy that both nickel and cobalt are usually present in meteoric iron—the average composition of meteorites being 90 per cent. of iron, 8 per cent. of nickel, and 0·5 per cent. of cobalt, curiously enough often with a trace of the other feebly magnetic metals, manganese and chromium.

This uniform coincidence in the properties of iron, nickel, and cobalt, suggests the practical inference that nickel and cobalt might be obtained in a malleable and ductile condition when submitted to a process similar to that by which wrought iron is produced. At present it is impossible to procure nickel or cobalt wire, though there seems no reason why they could not be made if a demand arose. Nickel wire would probably prove very useful from its high tenacity and comparative freedom from oxidation.

The following Table sums up some of the most striking points of contact in the physical properties of the three magnetic metals *par excellence*.

Table showing the Physical Relationship of the Magnetic Metals.

Substance.	Density. Water =1.	Atomic weight. H=1.	Specific heat. Water =1.	Atomic heat.	Dilatation		Conductivity		Tenacity and melting-point.
					by heat*.	by strain*.	for heat. Silver=1.	for sound*. Air=1.	
Iron ...	7·8	56·0	0·1138	6·38	·0926	·0387	·168	15·3	Very high.
Nickel.	8·3	58·5	0·1091	6·33	·0899	·0394	·131	14·9	„
Cobalt.	8·5	58·5	0·1070	6·26	·0981	·0436	·172	14·2	„

From this Table it is evident that the molecular constitution of the magnetic metals is essentially alike, largely differing from bodies which are not magnetic. And this being so, further evidence is afforded that the evolution of ordinary magnetic phenomena is in some way associated with the peculiar and similar structure of the molecules of iron, nickel, and cobalt.

* For the figures in this column I am indebted to a paper by M. A. Masson, in the *Annales de Chimie et de Physique* for 1858. In the heat column the decimal would, of course, have to be moved four places to the left to express the coefficient for 1° C. The dilatation by strain was of one metre of the body under a weight equal to itself.

LXIV. *Intelligence and Miscellaneous Articles.*

ON VARIOUS CASES OF INTERMISSION OF THE VOLTAIC CURRENT. BY A. CAZIN.

IN pursuing the researches which I have undertaken on the heat of electromagnets, I have had occasion to observe several cases of intermission of the voltaic current, which, I believe, have not yet been brought into notice.

Experiment 1.—A voltaic circuit is formed of 20 average Bunsen elements and a coil of 960 turns containing an iron tube 8 centims. in diameter and about 1 millim. thick. It can be closed or opened at will by means of a platinum point and a layer of mercury, which communicate respectively with each of the rheophores.

When the platinum is not in contact with the mercury, and they are put into communication with the armatures of a condenser with a glass plate (armed surface 3 square metres), a continued rustling is heard in the iron nucleus. The same effect is produced when, the condenser being omitted, a layer of alcohol is interposed between the mercury and the platinum point. The noise ceases when the alcohol is removed so that the platinum and mercury are separated by a layer of air, and also when the point is dipped into the mercury.

These facts indicate that the current passes through the glass in the first case, and through the alcohol in the second, and that *its passage is intermittent.* The nucleus of iron undergoes a rapid succession of alternate magnetizations and demagnetizations; and each demagnetization occasions a faint sound in the nucleus. The rapid succession of these noises constitutes the rustling which is heard.

When the iron nucleus resounds, a galvanometer indicates only a continuous current. In fact it cannot indicate any thing else when the intermissions are very close to one another.

I think that the cause of the intermission is the condensing action of the glass and the alcohol; when the two faces of the insulating body, which are in contact with the rheophores, have acquired a certain electric potential, a discharge takes place through the insulating layer; the magnetism of the nucleus increases during the charging of the condenser, and diminishes during its discharge; the noise is produced during the diminution of the magnetism; after each discharge a certain time elapses before the condenser is recharged; and the same phenomenon is reproduced indefinitely.

It is readily ascertained that the nucleus resounds during the diminution of its magnetism: we have only to immerse the platinum point in the mercury, and then withdraw it; at the moment when the spark issues at the point of interruption, a relatively intense sound is heard in the nucleus. It is solely the rupture of the cir-

cuit that gives rise to this sound; the closing produces no effect, at least in my apparatus.

M. de la Rive, in 1843, discovered that a current *interrupted by means of a rheotome* produces a sound in the iron of an electromagnet; but I think that the phenomenon I have just described has not been previously signalized.

Listening for a sound in the nucleus of an electromagnet may be regarded as a new process of investigation; we have seen that it reveals the intermission of the current in circumstances in which the known methods are insufficient. I will mention an instance which shows that this method accords with the others when they can be employed simultaneously.

Experiment 2.—When the spark from the rupture of the preceding circuit is observed with the aid of the revolving disk, after the manner made known by me to the Academy on the 7th of April last, the spark appears *compound.* When it bursts in alcohol, and the platinum point and the mercury are connected with the armatures of the condenser, the revolving disk shows that the spark is composed of four or five successive bright strokes; the intervals between the strokes diminish from the first onwards. (I have already described the division of this spark by another process, *Bulletin de la Société Philomathique*, 13th May, 1865; and the journal *l'Institut*, 31st May, 1865.)

The sound produced by the rupture-spark presents a similar mode of division; it is the same with that which is heard in the condenser, and with that which takes place in the nucleus: these three sounds are *composed* exactly in the same manner.

The production of a sound in the condenser proves that there is a partial discharge through the insulating substance, although at no part does this appear to be pierced.

The sound of the condenser is augmented by increasing its surface, up to a certain limit which cannot be exceeded. At the same time the spark between the mercury and the platinum point is seen to diminish. These modifications indicate a change in the distribution of the electricity—which could be mathematically analyzed by regarding the alcohol of the interruptor as the insulating plate of a second condenser united to the first by the armatures of the same sign.

I think there is no essential difference between the intermission of the current which accompanies the breaking of the circuit in the circumstances I have just described, and that presented by my first experiment.

Experiment 3.—The platinum point of the mercury interruptor is screwed into a fixed nut, so that it can be raised or lowered. The mercury and the point communicate respectively with the armatures of a condenser of 1 square metre surface. The rest of the circuit is arranged as before.

The point being dipped into the mercury, is gradually raised until

the spark springs through the alcohol. The point then remaining fixed, a succession of sparks is set up and persists for a long time. These sparks are lively and sonorous, and can readily be counted.

The level of the mercury visibly oscillates beneath the point. A possible cause of the oscillation is:—the spark being formed by the vapour of the mercury, the elastic force of this vapour depresses the level of the liquid; this returns to its former level, passes it in virtue of its acquired velocity, and rejoins the platinum point. Falling again, the mercury produces a fresh interruption, and the same phenomenon is repeated.

This purely mechanical cause cannot be the only one; for the circumstances favourable to this new mode of *automatic interruption* are those which accompany the decomposition of the rupture-spark into a small number of successive bright strokes. This correlation is recognized by changing the extent of the condenser, which alters the number of the divisions of the spark. Thus, on continually diminishing the surface of the condenser, the sparks are seen to follow one another more and more rapidly; and finally, when the condenser is omitted, there is only a crepitating voltaic arc. It is probable that the oscillation-period of the mercury comprises a definite number of intermissions in the discharge of the condenser, and that these two causes are in mutual dependence.

I believe that the discharge through air, under the form of a voltaic arc, and the discharge through glass, of which experiment 1 supplies an example, are comparable, and that the well-known crepitations of the voltaic arc are due to the same cause as the phenomena of which I have just spoken. All these facts may be brought into connexion by a single proposition: *the insertion of a suitable resistance in the voltaic circuit determines the intermission of the current.* The laws of the intermission will have to be studied with the interposition of a condenser, because the periods are long enough to be observed with facility. The laws found in this manner will afterwards be generalized, and must conduct us to the known laws of the currents which are regarded as continuous.

We cannot omit to derive from the whole of these considerations the important conclusion that *the current is a succession of modifications accomplished periodically in the circuit.*—*Comptes Rendus de l'Acad. des Sciences,* vol. lxxvii. pp. 1095–1098.

EXPERIMENTS ON EVAPORATION. BY M. STEFAN.

Atmometric experiments have hitherto led to no results expressible in the form of laws. The conditions under which they were made were not simple enough; yet they were sufficiently varied. The science of evaporation, especially of the diffusion of vapours, remained an unoccupied field.

In the experiments whose results are here given, the more volatile liquids, and in the first place ether, were employed. In order to avoid the great lowering of the temperature at the surface, narrow tubes were chosen for evaporating-vessels, instead of the wide vessels hitherto usual.

1. *The velocity of the evaporation of a liquid from a tube is inversely poportional to the distance of the level of the liquid from the open end of the tube.* This law holds with rigorous exactness when the distance of the level a little exceeds 10 millims.

2. *The velocity of the evaporation is independent of the diameter of the tube.* This result was obtained from experiments with tubes the diameter of which varied from 0·3 to 8 millims.

3. The velocity of the evaporation increases with the temperature, so far as with this the vapour-pressure of the liquid rises. If p be the maximum of elasticity of the vapour corresponding to the temperature of the observation, P the atmospheric pressure under which the liquid evaporates, *the velocity of the evaporation is proportional to the logarithm of a fraction of which* P *is the numerator, and* $P-p$ *the denominator.* If the pressure of the vapour becomes equal to that of the air, this logarithm becomes infinitely great, and signifies that under this condition the liquid boils.

Experiments were also made on evaporation in closed tubes.

If the open end of a tube, the other end of which is closed, be dipped in ether, bubbles form and issue continually from the tube, and, *at first, the times in which successively equal numbers of bubbles form are proportional to the odd numbers.*

If the immersed tube contains hydrogen instead of air, the same number of bubbles form in one fourth of the time. *Evaporation proceeds in hydrogen four times as rapidly as in air.*

The same result was also furnished by an experiment in apparatus in which a liquid can be brought to evaporation in an open tube in various gases. It consists of a T-shaped glass tube; into its vertical arm enters the tube containing the liquid to be evaporated; through the horizontal cross-piece a continual current of the gas is conducted.

If a tube provided with a cock be dipped with the cock open in ether, the level of the liquid within the tube will sink below that outside; and, *at first, the depths to which the interior level sinks below the exterior in definite times are as the square-roots of those times.*—*Sitzung der math.-naturw. Classe der Kaiserl. Akad. d. Wissensch. in Wien*, Oct. 23, 1873.

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PRINTED BY TAYLOR AND FRANCIS,
RED LION COURT, FLEET STREET.

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